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PREFACE

Among the social and biological sciences, psychology holds an enviable place in respect to the wealth of original articles published annually by its members. These written products of research are the bone and sinew of a growing science.

Enviable also, are the unparalleled provisions now in effect for the distribution of psychological journals to professional psychologists at comparatively low costs. When complaints are voiced, they are likely to reflect the reader's dissatisfaction with his own efforts at keeping up with the journal literature. Merely having journals readily available, at favorable club rates, is not enough. The time-consuming task of locating and assembling that which is of vital interest to oneself still remains. For this purpose, the *Annual Review of Psychology* meets a real need since it annually examines and reviews noteworthy papers appearing in approximately eighteen fields of psychology. Through these reviews, the scientists assist each other in keeping abreast of current literature which is of enough significance to deserve being incorporated as soon as possible in their work as teachers, counselors, clinicians, or investigators.

Volume I of the *Review* has been cordially received by those who have communicated with members of the Editorial Committee both in our own country and in other countries wherein psychology plays a prominent role in universities and in practical affairs. The Editorial Committee wishes to acknowledge its indebtedness to the authors of chapters in Volume I, whose pioneering efforts largely insured the cordial reception accorded the inaugural volume. The Committee believes that the excellent work of authors of the reviews in Volume II will insure a similar cordial reception for the volume presented herewith.

The Editorial Committee deems it noteworthy that overlapping has proved to be somewhat less of a problem than had been expected. Although two or more authors may cite the same paper, the reasons for the citations usually differ and the uses made of their contents are not the same. Actually, some cross-referencing through overlapping is desirable because of the intricate ramifications of much worth-while data of psychology.

The Committee is particularly grateful to scholars who displayed their interest in this undertaking by suggesting new ways in which the usefulness of the *Annual Review of Psychology* may be further enhanced. Frequently, changes in the method of listing articles in the bibliography have been mentioned. Some say that they should be arranged alphabetically and that complete titles of articles as well as textbooks should be given. Some recommend that the latter practice be followed only when the number of references is not so great as to usurp space needed for more substantial reviews of the articles themselves. And quite appropriately, some have suggested

changes in the methods of marketing the volumes in order to make them more readily available to college teachers and graduate students through the university bookstores. These and other suggestions have been passed on to the Board of Directors of Annual Reviews, Inc., with recommendations that as many of them as feasible be adopted.

J.E.A. C.I.H.
J.G.D. J.G.M.
C.H.G. D.W.T.
C.P.S.

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CHILD PSYCHOLOGY

By Roger G. Barker

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A conspicuous feature of child psychology in 1950 is the absence of a well-defined body of literature to which a reviewer can turn. Psychological studies of children are found in almost every specialized psychological journal and in sociological, anthropological, medical, educational, biological, and other professional publications as well. This has more significance than the added work it causes the reviewer. It strongly implies that current research on children is centered on fundamental problems of behavior rather than on children per se. It suggests that children are seen by investigators of many disciplines as suitable material for studying a wide variety of behavior problems. It indicates, too, that social and biological scientists feel competent to work with children if the problems under consideration will profit thereby. There is little evidence of the existence of a professional group of skilled child behavior specialists.

To the extent that the dispersion of child psychology throughout general psychology and other social and biological sciences indicates a breakdown of archaic or arbitrary subject matter divisions of these sciences, it is an encouraging sign. It is a good indication, too, if it means that the behavior of children is being studied from many viewpoints. However, the question arises in the reviewer's mind as to whether the abdication of child psychology as a professional specialty cannot be carried too far. This question is stimulated, perhaps, by other conspicuous features of current child psychology.

Child psychology in 1950 is not only widely dispersed and nonprofessional, but, except as a teaching field and a therapeutic specialty, it lacks vigor. By every index available—number of publications, number of papers presented at scientific meetings, membership in scientific societies, and establishment of research institutes—child psychology shows little life. In a previous publication (1), the writer estimated that the rate of scientific investigation of children's behavior in 1938 was about one-third the rate of investigation in the single medical specialty of tuberculosis. The rate of scientific production in child psychology in 1950 appears to be considerably lower than in 1938, when approximately 500 scientific publications referring to children's behavior were indexed. In 1949, approximately 250 publications were indexed. At the 1949 meeting of the American Psychological Association three per cent of the sessions were devoted to child psychology, and children were used as subjects in 12 per cent of the investigations reported. The Division on Childhood and Adolescence of the American Psychological Association ranks eleventh among the 17 divisions of the Association in the number of Fellows enrolled; it ranks eighth in total size. There are more Fellows in the Division of Military Psychology than in the Division on Childhood and

Adolescence. A systematic perusal of news regarding scientific and academic activities during the period of this review has provided little evidence that new child research programs are being established. These same news channels reported rapid expansion of physical and biological research centers.

Furthermore, although a number of excellent studies have appeared, the average quality of current work in child psychology is not high. Numerous reports of investigations of important problems cannot be included in this review because they do not reach adequate levels of scientific investigating and reporting. There are doubtless a number of reasons for this. One is the extraordinary difficulty of the problems presented. Another is the fact that child psychology at the present time is between two methodological eras. Older, well-developed procedures are giving way to new but unverified methods. During such a transition it is inevitable that much work should fail to meet exacting methodological standards. These difficulties are transitory and, to a considerable degree, self-correcting. However, there is evidence of a less benign problem, namely, poorly trained personnel. The Children's Bureau of the U. S. Federal Security Agency (2) has prepared a revealing review of research in progress. While the survey is not complete, it is by far the most comprehensive overview of contemporary research in this field that has ever been made, and it appears to be an excellent sample of current work. Seven hundred and seventy-eight of the 1,605 projects reported are primarily concerned with the behavior of children and 827 of them with the physical development and health of children. Most of the reports give the academic degrees of the principal investigators. Seventy-four per cent of the investigators in the fields of physical development and health hold a doctor's degree (Ph.D., M.D., D.Ed., or Sc.D.); 20 per cent hold M.A., B.A. and miscellaneous special degrees, and in 6 per cent of the cases no degree is reported ($N=360$). Investigators of children's behavior report having the doctor's degree in 51 per cent of the cases, lesser degrees in 39 per cent of the cases, and no degree in 10 per cent of the cases ($N=1,145$). While this reviewer subscribes to the view that high academic degrees are only very roughly related to intellectual creativity and research ability, these data would make it appear, nonetheless, that research in child psychology in the United States is in the hands of less well-prepared persons than research in physical development and medicine. This is a problem deserving of careful consideration. For example, it raises the question as to whether greater professionalization of child psychology might help to raise personnel standards.

It raises, too, the question of the resources available for research on children's behavior. The Children's Bureau survey provides some data that are indirectly relevant to this question. An analysis of the sponsors and sources of support of studies under way indicates that social agencies, universities, and government agencies are the most frequent sponsors and supporters of research at the present time. Public schools rank fourth in frequency of mention as sponsors of research on children's behavior; 16 per cent of the sponsors mentioned are public schools ($N=991$). Education departments in universities, colleges, and school systems are given as the professional affilia-

tion of principal investigators in 17 per cent of the cases wherein professional connections are reported ($N=956$).

To the reviewer, these are significant results. It is as if a reviewer of recent work on human embryology and the physiology of children were to find a small proportion of the relevant research being done by medical personnel, or supported by hospitals and medical schools. This may be an important reason for the generally meager quantity of work in child psychology today. When the greatest consumer of scientific knowledge in an area is one of the smallest supporters of fundamental research, in this case teachers, schools, and departments of education, it would appear almost inevitable that research resources would be inadequate, facilities poor, and personnel ill-prepared.

The low level of scientific output in child psychology in 1950 is in contrast with the amount of programmatic, didactic, and speculative writing. In professional publications covered by psychological, medical, and biological indexes, we have found, in the period under review, approximately three such publications for every one that reports original observations or experiments. This apparently means that there is a great demand by professional people for scientific information about children. It is, indeed, unfortunate that child psychology should be relatively inactive at a time when so much is expected from it.

In view of the indefiniteness of the boundaries of child psychology at the present time, it has been necessary to provide some limits for the purposes of the present review. Publications that fall within the following categories have been included: (*a*) studies that aim at defining the parameters of general psychological laws as they exist for children, i.e., studies in which the child rather than the psychological law is the primary focus, and (*b*) studies of functions that are unique to children or highly characteristic of them. The following have been excluded: (*a*) programmatic and didactic writings; (*b*) studies using clinical procedures, including projective methods.

One of the growing points of child psychology at the present time is the development of methods of describing and measuring the psychological situations of children. Earlier studies of environmental variables, such as presence or absence of siblings, ordinal position in the family, education of parents, and number of books in the family library, yielded disappointing relations with behavior. They have almost entirely disappeared from current research. Other situational variables have taken their places. We now find studies of the relation of behavior to the degree of warmth and indulgence in the home, to the amount of frustration and gratification at school, and to early feeding schedules. In accordance with this estimation of an important characteristic of child psychology at the present time, this review will be divided into two major parts, namely, (*a*) normative studies which describe the behavior of particular classes of children and (*b*) dynamical studies which investigate the conditions of behavior. In addition, one section dealing with theoretical writings and one mentioning surveys will be included.

Normative Studies

Perception, cognition, and conceptualization.—The comparative study of visual perception in children is an active area of research at the present time. Piaget (3) has investigated errors in the perception of the size of angles and the length of their sides in children, 7 to 12 years of age, and in adults. Using a standard angle as the constant stimulus, subjects selected from a series of figures of varying sizes the one that was equal (half, double, or triple) the standard. Illusions were found to be systematically related to the acuteness and obtuseness of the standard angle and to whether the figure was an open angle or a closed triangle. Children tended to be more variable in their judgments than adults, but the magnitudes of the illusions were not consistently greater in the child subjects. An extensive theoretical discussion of the general perceptual principles involved in illusions of this sort follows presentation of the data. Dixon (4) found that when children are able to respond correctly to the words "big" and "little" they also respond "correctly" to the ring-segment illusion and recognize the "error" in the behavior when the segments are reversed. The correlation between ability to respond correctly to the words denoting size and recognition of the "error" in response to the illusion was $r = .79$. Dixon interprets these findings as supporting the theories of Spearman that the eduction of relations is an essential characteristic of the cognitive process. Meister (5) compared children who were from four to six years of age with adults in ability to equate from memory visually perceived geometrical figures with a standard. He did the same with background pattern. He found that children equate larger figures to the standard figure than do adults; he found no difference in adults and children with respect to background pattern. Meister interprets his results to mean that children's perceptions of figures are more diffuse, more poorly articulated and less cohesive than the figure perceptions of adults, but that the perceptions of background remain in a primitive, undifferentiated state throughout life. Pronko and associates (6) studied the color perceptions of kindergarten, first, second, and third grade children by means of the Dvorine charts. They found that color naming increases in correctness from kindergarten to the third grade, that there are no sex differences in color naming ability, and that color blindness appears to have a developmental course during these ages. Unfortunately, the important volume by Gesell, Ilg & Bullis (7) on the development of vision in children became available too late for detailed attention in this review.

Children's conceptions of the origin of babies were investigated by Conn (8) by a "play interview" method in which the child subject spoke for a doll at the instigation of the experimenter. He found that children under six years of age ascribe the origin of babies to nothing nearer the truth than "the hospital" and "the doctor"; the mother's role is not mentioned. One-third of the children six to nine years old mentioned the mother as being involved in some way. After nine years of age the child brings the father into the picture, although his role is stated with great uncertainty. Nagy (9) studied Hungarian children's conceptions of death. He used a nondirective interview

in the case of children from three to six years old, and written compositions and an interview in the case of children from seven to ten years of age. He found a developmental trend from the age of three, when children deny death as a regular and final process, through stages when death is said to be temporary, with imaginary ghosts and angels involved, to a conception of death as a natural, inevitable termination of life.

The above-cited experiments of Piaget, Dixon, and Meister, the first two pointing to the completeness of some phases of cognitive processes at an early age and the latter to the primitive and undeveloped nature of other aspects, indicate the possibility of very fruitful research on complex perceptual and cognitive processes in young children. Methodological problems of crucial importance arise in connection with the above-mentioned investigations of Conn and Nagy. Birth and death are among the most difficult intellectual problems that confront men and they cause intense emotional reactions. It is very difficult, therefore, to get at children's real conceptions of these matters. Conn's method is an important attempt to avoid emotional resistance. However, the difficulties which arise from confronting children with problems beyond their powers of comprehension still remain. Isaacs' (10) criticism of Piaget's early methods would appear to apply to Conn's procedures, too. These comments apply much more strongly to Nagy's study. One wonders what a group of adults would produce if suddenly asked to write "Everything that comes into your mind about death"; in any case, one would hardly expect to secure rational concepts regarding death.

Attitudes and interests.—Luchins & Luchins (11) studied the attitudes of fourth, fifth, and sixth grade children toward being placed in a "dull" and a "bright" section of a school class, segregated on the basis of intelligence test results. Attitudes were secured by direct interview. Ninety-six per cent of the bright children, 87 per cent of the average children, and 75 per cent of the dull children expressed a desire to be in a "bright" section. It was clear that a decided stigma attached to being placed in a "dull" section, although the school authorities (and the experimenter) attempted to make the significance of the grouping secret, by designating "dull" sections as 4^1 (5^1 and 6^1) and the "bright" sections 4^2 (5^2 and 6^2). Wolf & Fiske (12) studied by direct interviews the attitudes of children from 7 to 17 years of age to comic books of various sorts. The children reported that their interests changed from a preference for animal comics at the younger ages to a preference for adventure and true comics at the older ages. Jurovsky (13) analyzed the written responses of 18 and 19 year old Czechoslovakian subjects to the questions, "What is your father's (mother's) relation to you and yours to him (her)?" Girls reported a better relationship with both the father and the mother than boys; boys and girls reported a better relationship with the mother than with the father. Beller (14) presented 9, 12, and 15 year old boys with verbally described situations in which dishonest behavior was possible. The boys were to indicate (*a*) what they would do in each situation, and (*b*) the right thing to do in each situation. Beller found that the "honesty" of the anticipated behavior dropped from 9 to 15 years of age, but that the

"honesty" of the desirable behavior increased from 9 to 12 years of age with no change from 12 to 15.

Jersild & Tasch (15) made an extensive investigation of the interests of 2,248 children in grades 1 to 12 from a wide variety of cities and towns in the eastern and midwestern United States. Children above the third grade wrote upon such questions as "My three wishes," "One of the happiest days of my life," "What I like best (least) in school." There were 11 questions. Children in the first three grades were questioned orally. The investigators do not claim to get at children's deep motivations by this method, but they do believe that the manifest interests of children are of importance in themselves and are of practical significance for many problems of education and community planning. Only a few of the many findings can be mentioned here: there is evidence of a steady decline with age in educational morale; topics under the general heading of social studies are mentioned unfavorably; marked differences between communities occur in the extent to which children express friendliness and good will toward one another or express ill will, hostility, fear, and other evidences of social friction; superficial, unproductive, boring activities occupy a great amount of the in-school and out-of-school time of children. There are indications in this study that "open ended" questioning of children provides valuable information on a number of aspects of institutional and community morale as well as on the more traditional age and sex trends. The general effectiveness of the method is verified by Wallon's (16) work with French children. Using essentially the same procedure as Jersild & Tasch, he studied the ideas of 474 children, 3 to 15 years of age, concerning goodness and badness, honesty and dishonesty, and bravery. His results are very extensive and cannot be briefly summarized. This is, in fact, a weakness of this and the preceding study.

These studies attest to the great value of nondirective questionnaire and interview methods in providing data on the world as experienced by children. Their great weakness lies in their lack of conceptual schemes for systematizing the data and bringing them to bear upon crucial psychological problems. The data are interesting in the way that a poll-taker's distributions are interesting; they provide intriguing facts that sometimes are of practical importance as well. These data would appear to bring important grist to the mill of child psychology, and these investigations have done a good job of sifting it; however, they have not done much grinding. The lack of a conceptual mill for grinding such facts as these raises the question of their reliability and validity in acute form. If one wishes to study the verbalizations of the children as given, the methods of these studies are, of course, adequate; but if one is concerned with the consistency of verbal behavior and with its significance for the feelings and the overt actions of children, serious questions arise. Fundamental studies of the adequacy of interview and questionnaire methods with children of different ages are then required. This is especially true of normative, nontheoretical studies. Investigations of the dynamics of behavior often provide a kind of internal validation of the methods. When such studies involved a hypothesis that rests upon independ-

ent empirical or theoretical evidence, the verification of the hypothesis also substantiates to a degree the techniques used. In the case of purely descriptive and normative studies, however, such indirect validation of methods cannot usually occur.

In spite of the fact that much remains to be done in this area of studies, such work as that by Jersild & Tasch and by Wallon indicates that we have come a long way since G. Stanley Hall studied the contents of children's minds. We have learned a great deal about how to question and interview children.

Motor behavior.—Balint (17, 18) devised an instrument for recording pressure changes within an infant's nursing bottle during feeding; a polygraph record indicated the rate of sucking with great sensitivity. He collected data on 100 infants varying in age from birth to nine months. Balint found four kinds of sucking rhythm: (*a*) a basic frequency, ranging from 8 to 20 sucks per ten-second interval (the basic frequency was highly consistent for a particular infant), (*b*) a "restarter" frequency, ranging from 13 to 35 sucks per interval (this frequency usually occurred after a pause), (*c*) a second basic frequency of 15 to 30 sucks per interval (this frequency sometimes occurred when the infant was annoyed), (*d*) a rapid frequency called "quavering," ranging from 40 to 100 sucks per interval. Quavering occurred during pauses and was sometimes superimposed upon the basic frequency. Balint found no sex differences in sucking rate and little indication of an age difference. This method appears to have great promise as a means of studying an important motor performance of infants. Ames (19) studied the frequency of smiling of children from 18 months to four years of age in a nursery school situation. At 18 months the children averaged one smile every 6 minutes and at four years one smile every 1.3 minutes. At the younger ages children smiled most frequently at their own motor activity, such as falling down on purpose, bouncing, or sliding; at four years, behavior with other children produced the most smiles. However, at every age observed the child smiled at his own actions more frequently than at the behavior of others. Ames (20) also reported upon changes in type of laterality which takes place during the first 10 years. She found that periods of bilaterality alternate with periods of unilaterality and that outstanding periods of bilaterality occur at from 24 to 32 weeks.

Constancy of intelligence and personality during childhood.—Three important studies of the constancy of behavior over long periods of time are reported. Bayley's report (21) on the intelligence of 40 children tested at 38 ages, from the first month of life to the 18th year of life, is impressive evidence of remarkable vision, persistency, and careful work. In this publication, Bayley is concerned largely with the variability of the test scores of her group of subjects at different ages, and with the correlation of the early and late test results. She presents evidence that changes in the standard deviations of the IQ's are not a chance matter nor due to the tests used. She finds two periods of clear-cut restriction in the variability of tested intelligence: near the end of the first year and after mid-adolescence. She suggests that fluctuations in variability are due to changes in the maturity of the functions

tested at different ages. She postulates that the greatest homogeneity in scores occurs when the functions tested are beginning to develop, that scores are most dispersed when the functions are growing rapidly but before any subjects have reached maturity in them, and that restriction in variability occurs as more and more subjects reach maturity. Bayley studied the constancy of mental test scores by test-retest correlations, by individual age curves, and by measures of the variability of the scores of the same individual on repeated tests. These data agree that test scores are unstable during infancy but become sufficiently stable by school age that prediction of the general intelligence level can be made fairly well. For example, test scores at four, five, and six months (average of three tests) correlate with test scores at other ages as follows: for 7, 8, and 9 months, r is .72; for 18, 21, and 24 months, .23; for 42, 48, and 54 months, −.16; for 8, 9, and 10 years, −.06; and for 17 and 18 years, .01. Other sample data are given in Tables I and II. Although many children maintain a fairly consistent level of intelligence after six years of age, in some there are wide shifts in mental level. A number of illustrative individual curves is given.

Skodak & Skeels (22) made a fourth and final report on the intelligence of 100 children placed in foster homes under six months of age. The mean age at the time of the final test was 13½ years (range 11 to 18 years); three previous tests were given at the approximate mean ages of two, four, and seven years. In general, the data of Skodak & Skeels confirm Bayley's findings regarding the stability of test scores over a wide time span. Comparable data from the two studies are given in Tables I and II.

TABLE I

COMPARATIVE DATA FROM SKODAK & SKEELS AND FROM BAYLEY

Means and Standard Deviations of IQ at Different Ages

Test	Age at time of test*	Test used	Mean IQ	S. D. IQ
I S. & S.	2 yrs. 2 mo.	Kuhlmann-Binet and Stanford-Binet, '16	117	13.6
B	2 yrs. 0 mo.	California, Press	109	13.3
II S. & S.	4 yrs. 3 mo.	Stanford-Binet, '16	112	13.8
B	4 yrs. 6 mo.	California Pre Sch.	115	15.2
III S. & S.	7 yrs. 0 mo.	Stanford-Binet, '16	115	13.2
B	7 yrs. 0 mo.	Stanford-Binet, '16	123	15.1
IV S. & S.	13 yrs. 6 mo.	Stanford-Binet, '37	117	15.5
B	14 yrs. 0 mo.	Stanford-Binet, '37	130	19.2

* Subjects were tested almost exactly at the ages given in Bayley's study; in Skodak & Skeels' study the mean age of a seven-year range is given in each case.

TABLE II

COMPARATIVE DATA FROM SKODAK & SKEELS AND FROM BAYLEY

Correlation between Tests at Different Ages*

	Test II		Test III		Test IV		Test V
	S. & S.	B	S. & S.	B	S. & S.	B	B
Test I	.54	.49	.44	.50	.35	.45	.55
Test II			.70	.82	.58	.70	.62
Test III					.75	.87	.86
Test IV							.96

* The ages of Skodak & Skeels' subjects at each test are as given in Table I, whereas the ages of Bayley's subjects for Table II are as follows: Test I, 18, 21, and 24 months (average of tests at the ages given); Test II, 42, 48, and 54 months; Test III, 5, 6, and 7 years; Test IV, 14, 15, and 16 years; Test V, 17 and 18 years (average of two tests).

The solution to the main problem of Skodak & Skeels' study, namely, the effect of relatively superior foster home environments upon the intellectual development of children whose true parents were of lower social origins and whose true mothers were of inferior intelligence, is not greatly advanced by this final report. The fact is, as the authors point out, that insufficient data on the true parents and the foster homes of these children were available to the investigators. The reviewer agrees with the authors' conclusion:

> Perhaps the most important contribution this study can make . . . is to point out . . . the necessity of formulating more clearly the various criteria used in the selection and assessment of the foster homes and children. It is clear that the objective data used here, education and occupation, do not represent the real bases of selection and are not closely related to the child's mental development.

Because of this and other pioneering studies, we now know the requirements to be met and the pitfalls to be avoided in carrying through an adequate investigation of this problem. The tragedy will arise if the total resources to do the adequate job are not available. These investigators were justified at the time in doing their best to turn a service project into an undertaking that would provide some evidence in an almost unexplored area. But the days for makeshift research in this area are past. There is every indication that this important social problem could be solved and understanding greatly increased at an expense of not more than $500,000.

Neilon (23) compared personality sketches of 15 children which Shirley made during the first two years of the children's lives with sketches prepared independently 16 years later on the basis of personality tests, a standardized interview on attitudes, interests and activities, and ratings by parents. Obvious, identifying cues were eliminated from both series of sketches. Judges

were able to match the infant and adolescent personality sketches far better than chance. The mean of the judges' correct matchings of the personality descriptions of the five girls was 3.2 and of the ten boys, 2.6. Analysis showed much greater ease of matching for some children than for others.

These studies add important new evidence to that available, and to common experience, indicating that behavior is both persistent and changing, over long periods of time. Bayley adds to our knowledge of what changes and what persists. The stage is set for determining the sources of persistence and of change with more precision.

Miscellaneous normative studies.—Despert (24) investigated the sleep of 58 children aged two to five years by means of parents' reports and by observation in a nursery school. She found marked differences in the amount of sleep of children. In general, the children of this study slept better in the group situation at school than they did at home. Despert (25) also studied the dreams of 43 children aged two to five years. Her data are based on reports of parents and on daily records of the remarks of the children in free play and during play interview sessions. On the basis of 190 dreams reported by 39 children, Despert was able to summarize her findings as follows: Unpleasant dreams far outnumber pleasant ones; the dreams contain no direct sexual references; parents usually appear in benevolent roles; not a single instance of direct parental hostility occurs; no relationship is found between dream content and actual traumatic experiences. These results refer to the directly reported content of the dreams of the children. Unmentioned dream material that might be discovered by special techniques and the hidden significance of the content reported are not considered in the study. Ilg and associates (26) describe the $3\frac{1}{2}$ year old child in terms of his typical pattern of behavior. No numerical data are given; characteristic traits which have been observed in three-and-a-half-year-olds are listed. The authors suggest that $3\frac{1}{2}$ is an age of considerable disequilibrium. Smith (27) found the language development of children in bilingual homes in Hawaii to be retarded. Lehman (28) lists and documents with quotations from reliable sources outstanding achievements of persons in middle and late adolescence in science, writing, music, painting, philosophy, chess, and practical invention; 91 persons are listed.

Dynamic Studies

Studies relating behavior to physical conditions.—Ruja (29) found no correlation between amount of crying in neonates and length of labor at birth as measured by length of interval from the mother's admission to hospital to the child's birth. He interprets this as failing to support Otto Rank's hypothesis concerning the birth trauma. Phillips, Berman & Hanson (30) report a carefully controlled investigation of 101 children who had poliomyelitis one year previous to the study. These children were compared with their own, pre-poliomyelitis test scores on the Stanford-Binet, and with a control group of subjects upon specialized intellectual tests and a personality inventory. Four cases of crippling occurred in the group; 15 of the subjects

were out of school for one year following their illness. A mean decrement of 1½ points from the mean of pre-poliomyelitis Stanford-Binet IQs was found in the clinical group, which, in comparison with a gain of 2 points in the control group, approached statistical significance. The greatest loss occurred in the younger boys, six to eight years of age. The specialized, intellectual tests and the personality inventory did not differentiate consistently between the clinical and the control groups. There was some indication that the clinical type of poliomyelitis was more closely related to amount of deficit than was length of hospitalization. The authors point to the need for studies over a longer period of time following the acute phase of the illness and for behavior observations in natural situations. The reviewer would support both of these suggestions, particularly the latter. Psychologists have to avoid two kinds of futility; one is methodological inadequacy and the other is psychological insignificance. It is well to remember in this connection that tests of statistical significance are not also tests of psychological significance. The reviewer cannot avoid wondering whether a mean loss of 1½ points (or 3½ points) in IQ in a group of 101 subjects has behavioral significance in the world of children: to the child, to his peers, to his parents, and to his teachers; in school and at home. It is unfortunate that we have no simple operations for testing the psychological significance of a difference. The authors' proposal (30) for field observations and ratings by parents, teachers, and companions appears to be one way of approaching the question of the psychological significance of a statistical difference.

Studies relating behavior to factors in the broad cultural environment.— Joseph, Spicer & Chesky (31), as a part of an anthropological study, present test data on intellectual and personality characteristics of Papago Indian and white school children. The details of the differences in the Indian and white cultures and behavior cannot be reviewed here. In general, small differences in intelligence test scores were found. Differences in answers to questionnaires concerning sources of happiness, sadness, anger, shame, and fear, and concerning good and bad things to do were relatively large. This study is a good instance of the effective use of psychological tools in anthropological work. Appropriate instruments for intercultural comparisons of intelligence and values were used and the data are given in such a way that anyone is free to evaluate them in accordance with his own purposes. This is in marked contrast with the data on cultural and psychological environments. Here we find a series of statements about child rearing practices, for example, with no indication of their source or their generality. This is not a criticism of this particular study; it is mentioned as an example of the fact that methods of describing behavior are in some respects much further advanced than methods of describing the situations in which behavior occurs. Psychological tests have their limitations, as was mentioned in connection with the previous study (30), but they have their advantages, too. Some recent advances in methods of describing psychological situations are reviewed in the next section of this chapter.

Jouhy & Shentoub (32) report the only study that has come to the re-

viewer's attention of after-effects of the war upon children. A group of Jewish refugee children in France was studied by psychometric and clinical methods. The conclusion is reached that war experiences produced profound changes in the basic personality structure of these children.

The effect of group membership on the perceptions and attitudes of children has been the subject of two investigations. Radke, Trager & Davis (33) investigated by a picture-interview method the perceptions and attitudes of 250 kindergarten, and of first and second grade children regarding a number of minority groups. School and play scenes showing Negro and white children in situations identified as Jewish, Catholic, and Protestant were shown the children individually. The interviewer began with very general questions, such as "Tell me about the picture," but the identity of the pictured children and situations was made more specific by the subject as he responded to the experimenter's questioning. Radke and her associates found that the social world as perceived by children of kindergarten age is differentiated into racial and religious groups and that the children's knowledge of these groups and their attitudes toward them reflect the knowledge and attitudes of the adults with whom the children live. Age changes throughout these young children were in the direction of greater awareness of group conflicts, patterns of exclusion, and forms of derogation, and toward greater acceptance of prejudiced attitudes; there was no increase in accuracy of information about group differences. The authors found no evidence that prejudice in children of these ages is related to basic personality structure or individual security. The method of this study is particularly appropriate to the problem. It allows the investigator to raise only questions of which the child is already aware and it does this in the child's own words and on his own intellectual level. It reduces resistance by allowing the child to attribute his attitudes to a pictured person. Biber & Lewis (34) used a picture-interview procedure to study the attitudes of first and second grade children toward the teachers and routines of two schools: a traditional public school where the children came from lower social classes (70 per cent were Negro and 15 per cent Spanish-speaking, white children) and a private, progressive school where the children came from professional families. The responses of both groups of children indicated that they perceived their school situations in some respects as the experimenters and teachers did: as more free and genial in the private than in the public school. However, in both groups there was evidence of an "unrealistic" perception of school as requiring more blind obedience and as using more physical punishment than adults believed to be the case. Apparently there is either a broad, culturally determined meaning attaching to schools which marks them as disciplinary institutions irrespective of the individual's common experiences, or children project upon pictured school scenes their personal insecurities and guilt.

The problem of the relation between social class membership and the behavior and personality of children is the focus of considerable attention at the present time. Milner (35) studied intensively the behavior and life situations of 30 early adolescent, lower middle class, midwestern, American boys

and girls. She used 29 methods of collecting data, including interviews, psychometric instruments, attitude and interest questionnaires, written essays, ratings by adults and age mates, sociometric choices, and projective tests. From these data, 15 female-typical, 13 male-typical and 11 total-group-typical characteristics were found, leading Milner to conclude that sex and social class are subcultures of the American culture that condition behavioral development. These early adolescent children are reported to exhibit a pervasive anxiety that seemed to result from feelings of lack of affection; the boys were most concerned with problems of impulse-control and the girls with social conformity. The home situations of the most typical children of each group were studied; the evidence suggests that the home, particularly the mother, is the chief agent through which the culture acts upon early adolescents. This study contains a wealth of data and of suggestions. A methodological note is perhaps in order. The study lies midway between a clinical and a scientific investigation. The general procedures by which the raw data were reduced to general statements are described, but the data themselves are not reported. Identification of general procedures and operations is a step in the right direction; but ultimately it will be necessary to go further and provide both data and details of methods in order that the reader can evaluate the conclusions drawn and repeat the operations used. As studies of children come to grips with more and more difficult and important problems, this expository problem becomes increasingly difficult and its solution more urgent. As it stands, this study could hardly be repeated by another investigator.

MacDonald, McGuire & Havighurst (36) studied the leisure time activities of fifth, sixth, and seventh grade children of four social classes living within the same area of a large city. Data were secured from diaries kept at the investigators' request over a two-week interval. Class differences in participation in organized recreational groups and in some individual leisure activities were found. There were a small number of exceptional, lower-class children who participated with middle-class children in middle-class recreational organizations. The investigators conjecture that these children were learning attitudes and habits leading to upward mobility. Volberding (37) reports somewhat similar social class differences in the out-of-school behavior of children of a small city. Barker & Wright (38) studied the school and community participation of children in a small town. Their data were a detailed record of the activity of one middle class boy throughout an entire day and records of the positions of honor and responsibility held by school children of different social classes. Little evidence of class segregation in the school or out-of-school activities of grade-school children was found. Some factors in small towns which may operate to reduce social class influences on the extra-family behavior of children are discussed.

It has long been obvious, of course, that behavior—perception, attitude, action, and personality—is molded by the cultural environment. These studies begin to define with greater precision than heretofore the ages at which cultural influences become effective and the extent of the influence of

particular subcultures. These are important results. These studies also contain clues to the even more important question of the mechanisms by which the culture operates on the individual child. It would appear profitable for experimentalists to comb this literature carefully for leads that can be tested in the laboratory; investigators working on instruments for measuring the psychological environment should find this a source of promising scale items and descriptive categories.

In closing this consideration of group membership and the behavior of children, the important sociological studies by Warner (39), Hollingshead (40), and Bossard (41) must be mentioned. It is encouraging to note that the line between sociological and psychological work on children is becoming very thin.

Studies relating behavior to factors in the family situation.—Three studies of the relationship between early feeding practices and the subsequent behavior of children are reported. Sears & Wise (42) interviewed mothers regarding their child-rearing practices and the behavior of their children. The mothers of 80 children two to seven years of age were interviewed. In this report, special attention was given to age of weaning, method of weaning, frequency of behavior disturbances at time of weaning, and amount of thumb sucking. Age at time of weaning varied from two weeks to older than seven months; five cases were cup-fed from birth and hence were not weaned. Method of weaning varied from sudden to very gradual deprivation of breast or bottle. The severity of reported behavior disturbances at time of weaning was greater for children weaned at the later ages and for those weaned by the more sudden methods. Amount of thumb sucking was not clearly related to either age or manner of weaning. This investigation points to a realm of extremely important and extremely difficult research. Its importance is obvious; the difficulties may be exemplified by the present study. The authors were interested in determining whether retention of the sucking method of feeding for a relatively long period of time strengthens the oral drive as indicated by behavior disturbance at weaning. To make this determination, it is necessary to secure behavior data on infants *of the same maturity at weaning time* who have been fed by sucking for different lengths of time. The condition given in italic is necessary in order to eliminate the possibility that maturation of a "sucking instinct" might cause the weaning disturbance rather than the long sucking time. The converse data would be pertinent also: different maturity at weaning time and same amount of sucking. Either of these sets of conditions is very difficult to fulfill. Infants are not customarily placed on the breast or bottle at different ages. Klackenberg (43) raises yet another condition to be considered. He studied the incidence of thumb sucking in children of from four to six years of age by methods similar to those of Sears & Wise. He related thumb sucking to length of breast feeding, to amount of spoon feeding, to use of a pacifier ("dummy teat"), and to average duration of each feeding. Klackenberg's data agree with those of Sears & Wise that there is no relation between frequency of thumb sucking and age of weaning or amount of spoon (cup) feeding. However, Klacken-

berg found thumb sucking to be less frequent in the case of children who, as infants, were uncomplicated "long feeders" or had been provided with pacifiers. Klackenberg's data suggests that, whether or not sucking is a learned response or an instinctual drive, the need for it can be satiated either by long feeding or by a substitute "dummy teat." It should be mentioned that these two studies of thumb sucking, one carried out in Sweden and one in America, agree in finding that mothers report thumb sucking in approximately 40 per cent of children. Holway (44) related the amount of time nursery school children spend in fantasy and reality play with mothers' reports of the strictness with which infant feeding and training schedules had been followed. She found a correlation of .78 between amount of reality play and absence of strictness in feeding schedules. This is a striking finding and deserves further study. It is urgent that further data on the relation of early feeding practices to behavior be secured.

Spitz (45) investigated three kinds of autoerotic behavior in children under 15 months of age, namely, rocking, genital play, and fecal play. The infants studied were in a foundling home, in the nursery of a penal institution, and in their own families. They were observed for one-hour periods four times per week over a period of a year. At least one of the three forms of autoerotic behavior was found in 61 per cent of the children and more than one form in 12 per cent of the children. There were no clear-cut sex or age differences in occurrence of autoerotic behavior. Genital play occurred in 100 per cent of children in families, in 64 per cent of children in the nursery of the penal institution, and in 9 per cent of children in the foundling home. Following this lead, Spitz offers evidence in support of the hypotheses that genital play in the first year of life is one of many normal bodily activities and games; that a close and balanced mother-child relation is an important prerequisite for its development which is a part of the general, high developmental level of children in families. He found rocking, on the other hand, in children wherein the mother-child relation was neither completely absent nor well-balanced and close. In the penal institution many of the mothers were inconsistent in their approach, alternating intense outbursts of love and hostility. Spitz believes rocking in the first year of life is a symptom of the arrested ability to master objects and social relations of every kind and results from the inconsistency of the environment's emotional interchange with the child. Fecal play seemed to occur in infants whose mothers were psychotic or prepsychotic, and Spitz believes that it represents the distortion of a real object relation caused by the change in the mother's personality. Here are promising leads that deserve careful investigation. In another article published during the year, Spitz (46) elaborates and interrelates data previously published on development in extreme environmental situations. Lourie (47) investigated the occurrence of rhythmic behavior (rocking, in Spitz' terms) in children of a wider age range and found it to be frequently associated with periods of tension, frustration, and confinement.

Hostility in normal children from four to six years old is the subject of an extensive research by Korner (48). Her data consisted of (*a*) a play inter-

view in which the child was confronted with a common frustrating situation, (*b*) an interview with the parents concerning the child's customary behavior in the situation depicted in the play interview, and (*c*) nursery school teachers' ratings of adjustment. Korner found that expression of hostility in play and in real life was very frequent; that there was no consistent relation between hostility in play and hostility in home situations as reported by parents; that there was no relation between expression of hostility and rating of adjustment by teachers; that there was no consistent relation bebetween parental rejection and hostility; and that severe corporal punishment by parents was related to strongly hostile behavior in real life. Four patterns of hostility expression were described. This study raises many questions regarding the significance often attributed to the expression of hostility in clinical diagnosis and therapy. If these "normal" children are an indication, neither fantasy nor "real" hostility is a diagnostic sign. Furthermore, the study discounts any simple relation between parental "rejection" (as seen by an observer) and children's hostility. Apparently, lawfulness can be found only by reconstructing the child's total life situation in which parental "rejection" may be a minor or a major factor, or may be completely changed in its significance by the child's perceptions.

Two studies of the personal-social interrelations of twins have appeared. Burlingham (49) presents some notes made in a wartime nursery upon three pairs of identical and two pairs of fraternal infant twins. She gives concrete instances of competition, imitation, contagion of feelings, and mutual annoyance. Smith (50), on the bases of interviews, questionnaires, and observation in test and experimental situations, described the interpersonal relations of 32 identical and 29 fraternal twins. He found the intra-pair relations of identical twins to be more intimate than those of fraternal twins. Increasing age appeared to reduce the intimacy of both kinds of twins; male twins were less resistant to these age disruptions than female twins. These data are subsidiary to Smith's main concern with twin similarities in after-images and eidetic images which will not be reviewed here. These studies indicate an important direction for twin research to take, namely, the description and measurement of actual functional behavioral relations that occur between twins and of their relations to the genetics, personalities, social situations, and physiques of the twins.

Studies mentioned in this and the previous section of this chapter exhibit a common concern with situations in which behavior occurs and with their dynamic relations to observed behavior. It is apparent that there is a groping for methods of measuring aspects of the environment that are relevant to behavior. The next study to be considered is a major contribution to this effort. Baldwin, Kalhorn & Breese (51) present the culmination of a number of years of research at Fels Research Institute on methods of rating parental behavior and home atmosphere. Their monograph presents data on the construction, reliability, intercorrelations, and use of the 30 Fels Parent Behavior Rating Scales. The scales are designated in the tabulation below where they are listed in clusters that have been found to be intercorrelated in a

group of middle class, American families. The clusters are given in order of the importance they hold for the behavior of children in these families.

Warmth

Child-centeredness of home
Parents' approval of child
Parents' acceptance of child
Parents' affectionateness with child
Parents' rapport with child
Intensity of parents' contact with child

Democracy

Parents' justification of policies to child
Child's participation in family decisions

Intellectuality

Parents' striving to hasten child's rate of development
Parents' readiness to explain to child
Parents' understanding of child's abilities, needs, etc.

Indulgence

Parents' babying of child
Parents' protectiveness of child
Parents' solicitousness for child

Adjustment

Satisfaction, stability, achievement in home
Discord in home
Effectiveness with which policies are carried out
Overt parent-child conflict

Interference

Parents' tendency to make suggestions to child
Parents' approval and disapproval of child

Restrictiveness

Degree of freedom allowed child
Coerciveness of parents' suggestions to child

Clarity

Clearness with which parents' standards are stated
Consistency with which standards are enforced

Miscellaneous

Activity level of home
Smoothness of routine functioning of home
Sociability of the family
Parents' duration of contact with child
Severity of penalties
Emotionality of parent toward child

The scales have from five to seven well-defined scale-points to facilitate ratings. Good agreement of ratings by trained raters can be secured.

It appears to the reviewer that the problem of describing the psychological situations of children is as important and as difficult as the description of their behavior. Without doubt, as much work and as many different approaches will be required to develop adequate methods of measuring the environment as have been required to measure behavior. As a matter of fact, many of the problems met in testing behavior are also met in measuring the environment. For example, an important problem for both is the range of cultural variation within which a particular instrument is applicable. Could Joseph, Spicer & Chesky (31) have applied the Fels scales to the Papago Indians? The important point to make here is that we are at last getting away from nonpsychological categories such as education of mother, occupational level of father, and social class position of family in describing the psychological situation of children. The Fels scales come much nearer to psychological reality by means of such scale points as: "Home extremely bustling, busy, excited, tense"; "Parent and child spend entire waking day together"; "Parent-child clashes occur now and then but they are exceptional, superficial and mild." Furthermore, the Fels scales cover a wide range of intra-family conditions; they get at pervasive, interrelated "atmospheric" factors. In so doing they contrast sharply with methods that select an isolated family variable such as parental hostility, strictness of feeding schedule, or age of weaning to study in relation to children's behavior.

Use of the Fels scales in research is exemplified in a study by Baldwin (52) of the nursery school behavior of children coming from homes varying in warmth, democracy, and indulgence. Nursery school behavior was rated entirely independently of parental behavior. Democracy was found to be by far the most important of the three factors in accounting for variability of nursery school behavior. Children from homes rated high for democracy were rated high in nursery school on the following behavior characteristics: active, socially outgoing, hostile, domineering, friendly. They also received relatively high ratings on activities requiring intelligence, curiosity, originality, and constructiveness. Children from homes high in indulgence were rated as physically apprehensive and inhibited in the development of large muscle skills.

Two other methods of measuring psychologically relevant aspects of the environment are reported, neither of which has been studied as thoroughly as the Fels rating scales, but they appear to deserve further investigation as methods for child psychology. One method is a parental attitude test developed by Shoben (53). This consists of a series of 85 statements such as "spanking is a good form of discipline"; "most children have a need for privacy" with which the subject agrees or disagrees. With suitable weights, the test differentiated between a group of mothers of problem children and a group of mothers of nonproblem children. Barker & Wright (38, 54) exemplify the use of long, detailed "specimen records" as a method of describing behavior and situation simultaneously.

It is to be urgently hoped that the problem of measuring the psychological environment will be taken seriously and not allowed to slip in the back door as merely a practical clinical, social work or research technique! It should be possible to benefit by experience with behavior measurement and to avoid mere operationism. The one criticism the reviewer would make of the Fels scales is that they are too empirical. Unless warmth, democracy, etc., can be better conceptualized we will end in the same cul-de-sac we are in with intelligence and personality measurement: factors defined in terms of statistical operations that are so abstract that their psychological content cannot be designated.

Theoretical Writings

Theory in child psychology in 1950 is dominated by two trends. The first is the continuing controversy between proponents of psychoanalytic and nonpsychoanalytic viewpoints. This literature will not be reviewed here since it is concerned so largely with the data of clinical psychology. Crucial issues as they apply to conceptions of children are critically examined by Anderson (55). In so far as current research is guided by explicit psychological theory, psychoanalysis predominates. In fact, it can almost be said that the only comprehensive psychological theory of childhood that is influential at the present time is psychoanalysis. In consequence, psychoanalytically oriented research is, by and large, promising in its inclusiveness and its concern with basic problems. It is unfortunate that so much of it is executed on a primitive scientific level. This is undoubtedly due, in part, to the fact that many analytic concepts are untestable as stated. The investigator is faced with a dilemna: he may add precision to the concepts and open himself to the criticism that he has changed them in unjustified ways or he may undertake indecisive, ambiguous research. However, it will be clear from the preceding review that good progress is being made at a few points. The second trend in current child psychology theory is the application of learning theories to problems of personality development in children. Since the evaluation of this work requires a comprehensive consideration of learning theory, it cannot be reviewed here. However, the papers of Ausubel (56) and McCandless (57), since they apply particularly to psychosocial development of children, should be called to the attention of the interested student. Interesting theoretical aspects of changing child-rearing practices are discussed by Escalona (58).

Literature Surveys and Texts

The great number of didactic, polemic, and common sense writings cannot be reviewed here; many of them are excellent and serve important educational purposes. A number of surveys of general and special aspects of child psychology have appeared (59 to 68).

Summary

Child psychology in 1950 has encouraging and discouraging trends. The amount of work being done is in marked contrast with the brave words

that are expressed concerning the theoretical and practical importance of increased knowledge about children. A great superstructure of applied clinical psychology appears to be in the process of erection with little actual concern for the foundations that are said to be laid in childhood. While some excellent work is being done, a great amount of poorly designed and executed research is appearing. This is probably in some degree a reflection of the difficulty of the problems, but also it seems to be due to low standards of professional training and meager resources.

The newer trends are away from experimentation in the classical sense to observations and reports of behavior in naturally-occurring situations. This direction of movement appears to have great promise, but there is a need for systematic clarification of the conceptual and methodological problems involved. One preliminary attempt to define and clarify the issues of psychological ecology has appeared (54, 69). The need for conceptual and methodological clarification is increased by the spread of research in child behavior to social sciences other than psychology. A multitude of methods and viewpoints are being brought to bear upon children's behavior out of which new understanding will certainly arise. This process would be greatly aided by integrating concepts and theories.

One trend that is clearly discernible is the attempt to improve methods of describing and measuring the situations of children and to bring such methods nearer to the level of excellence of present methods of describing and measuring the behavior of children. New procedures for testing, interviewing, and observing children are also appearing.

Such classical problems as nature-nurture, thinking and language, developmental schedules, and fetal and neonatal behavior have little representation in current child psychology. One reason for this seems to be that the exploratory studies have been completed. The definitive studies which should be undertaken are completely beyond the resources of single investigators or of most institutions. The fact seems to be clear that many of the important problems of child psychology require more time, staff, and resources than are now available. Some means of concentrating larger and more continuous resources upon unsolved problems of child behavior are essential.

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LEARNING

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Although students of the learning process continue to be as ingenious as ever in inventing new procedures for testing their theories, they continue also to stress animal studies. We may assume that this tendency is partly a matter of tradition, but we must also agree that work with animals is definitely motivated in part by the possibilities of simplification and control. Be that as it may, this review must deal to a larger extent with animal studies than with those concerned with human learning, in order to reflect fairly the kind of progress which is being made in the field.

It also continues to be true that, in general, theoretical controversies about the learning process place the stimulus-response proponents on one side and "all comers" on the other. Hence, this review must assume the general form it did last year, although minor changes will be made because research on certain topics has not been actively prosecuted, and because other topics have become active or are deemed by the present author to be worthy of being brought up to date at this time.

The Principal Viewpoints

Reinforcement theory.—For orientation to the main body of this chapter, it is necessary once more to review the essential contrasts between the modern stimulus-response point of view, as typified by Hull (1) and by Miller & Dollard (2), and the field-cognition point of view, most consistently and actively defended by Tolman (3, 4).

For most stimulus-response theorists, all learning is due to the occurrence of reinforcement in (close) conjunction with a to-be-learned stimulus and the response to be associated with it. The central concept, and the one challenged by opposing theorists, is that of reinforcement. It is defined by those who employ it as either reduction of a primary drive state or stimulus, or occurrence of a (secondary reinforcing) stimulus which has developed its power to strengthen associations by virtue of its earlier association with some primary reinforcement (1).[1] It should be noted that these statements about the

[1] Although the main attention here is given to the need-reduction, or Law of Effect, version of reinforcement, it is important to classify Guthrie in this same general group of theorists (5). For Guthrie, reinforcement is the pairing of any stimulus or stimulus pattern with a response or response pattern. Since evaluation of this definition depends upon the demonstration of learning without concomitant primary or secondary reinforcement as defined above, and this condition is difficult of attainment, and also because Guthrie (6) employs such concepts as maintaining stimuli, which may be changed or removed by reward or punishment or the reactions thereto, no attempt will be made to treat his point of view separately here.

The distinction between stimulus-response (contiguity) and stimulus-response (need-reduction) viewpoints as to the nature of reinforcement will undoubtedly re-

exact nature of reinforcement are put forward by Hull as best tries, rather than as final conclusions [(1) pp. 80–83, 98–101].

Reinforcement, as defined above, is a conceptual descendant of the Law of Effect. Consequently, when proponents of reinforcement state that the ultimate criterion of whether something is a reward is whether it can be shown to influence learning, they are attacked for circularity. The weakness of this attack is shown by Meehl (9). His position may be summarized as follows. First, be believes, an empirical Law of Effect is widely accepted. For example, an investigator may find that if sunflower seeds are presented to a hungry rat which has just made a certain response in the presence of a certain stimulus, the probability of that response to that stimulus on a later occasion is increased. The sunflower seeds (or any stimulus having a comparable position in the paradigm) may be called the reinforcer. This is a definition pointing at an experimental circumstance. Next, a trans-situational reinforcer may be defined as one which will strengthen all learnable responses, i.e., responses in a wide range of situations other than the one in which the reinforcer originally came to be known as such. Thus, a trans-situational reinforcer may also be identified by pointing at observations in which the reinforcer has its effect independent of the particular response being learned. When the reinforcing agents are so defined, Meehl believes it is possible to state a "weak" Law of Effect, that all reinforcers are trans-situational, or a "strong" Law of Effect, that every increment in strength of an association involves a trans-situational reinforcer.

The "strong" Law of Effect is rejected by some investigators, notably those of the field-cognition group (see below), but not justifiably, Meehl believes, because of any circularity inherent in the logic of it. Certainly it is not this imputed circularity, but rather the factual basis of reinforcement, which concerns the stimulus-response theorists at the present time. For example, Wolpe (10), reasoning from physiological bases, has challenged the possibly dual nature of reinforcement as it is defined by Hull; by reducing all reinforcement to a central excitation process in the nervous system, he would do away with any distinction between primary and secondary reinforcement.

During the year a number of papers appeared which may be described as attempts to test, extend, or analyze various aspects of Hull's theories. One such paper, by Wilcoxon, Hays & Hull (11), deals with the decline in effective reaction potential [(1) Ch. XVI] during successive extinction trials. A technique previously used for scaling reaction potential during learning (12) was here applied to data secured from rats in which a simple locomotion response was extinguished to a latency criterion. The principal difficulty encountered was the possibility that the size of the scale unit of effective

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ceive attention in the near future. Attention should be called here to Sheffield's (7) vigorous defense of Guthrie's position, and to Estes' (8) development of a mathematical theory of learning which in general is a contiguity theory but specifically leaves the definition of reinforcement undecided.

excitatory potential is different for extinction than for learning data. A related study by Zeaman (13) was concerned with the validity of the scaling process itself. He tested the notion that it should be possible, using two different measures of response, to arrive at the same scaled function for growth of excitatory potential. Latency and running time were secured for the performance of rats in a straight runway. Then excitatory potential was scaled for these measures by the Hull method (12). Exponential functions resulted for both types of response measure, but the two curves differed appreciably. Zeaman, in interpreting this finding, suggests that latency and running time are probably not two aspects of the same response, but two different habits, thus making the outcome of his study less critical of the Hull position than it otherwise might be.

Another and quite different attack on this problem has been made by Schlosberg & Heineman (14). As one measure of performance they used reaction-times to monosyllabic words by college students; as the other measure they used proportion of students giving any particular response. It was discovered that log reaction time yielded a straight line when plotted against cumulative frequency. To quote the authors [(14) p. 241],

> . . . either the logarithmic function, or the negative power function which was found to fit the present data almost as well, conforms surprisingly well to those which resulted from the elaborate treatment developed by Hull and his associates. On empirical grounds, there is little basis to choose one of these functions rather than the other.

The authors go on to criticize the role assigned to oscillation of reaction potential by Hull and his colleagues [(12); p. 240] according to their use of the variable, J-shaped distributions of reaction latencies should occur; this expectation is clearly not in accord with the facts.

In a series of three studies (15, 16, 17) Reynolds has been concerned with the working hypothesis used by Hull in relating magnitude of reinforcing agent to performance [(1) p. 128]. Hull suggested that the limit of associative strength attainable with an unlimited number of reinforcements is a positive growth function of the magnitude of the agent employed in the reinforcement process. Reynolds' own data partially supported this hypothesis, but only for the learning of responses which themselves were "replications" of a consummatory response. By contrast, where a nonconsummatory measure of learning was employed (as frequency of choice, or bar-pressing), rats given small or larger amounts of food reward did not learn at measurably different rates.

A technical point not settled by Reynolds concerns the question of whether limit or rate-of-growth is the aspect of the learning curve affected by variations in amount of reinforcement. Zeaman (18) provides direct evidence. He measured the latency of a running response of rats in a straight runway, amount of food reward being varied over seven steps in the various experimental groups. The latency data gave clear support to Hull's tentative statement that the limit of the learning curve, not the rate-of-approach to that limit, varies with differing amounts of reward.

Field-cognition theory.—The principal exponent of the field-cognition viewpoint is, of course, Tolman (4, 19). According to him and his associates, learning is a process of associating various internal or external stimulus events with each other, rather than with responses. As learning proceeds, a certain cognition (knowledge of object or event) becomes capable of arousing an expectancy that certain behavior will lead to another cognition. To stress what he conceives to be the interdependence of these three, Tolman used the hyphenated term, sign-gestalt-expectation. The organism is thought (3) to build up a "map" of its environment, capable of being "consulted," and guiding performance, at any subsequent time.

The conditions governing or limiting the development of cognition are those common to other points of view—physiological conditions, motivating states, practice, etc.—except that rewards or punishments have had no special or essential status in Tolman's system, nor has need-reduction. Rather, rewards and punishments, like other cognitive facts, may function as informers about environmental circumstances or sequences, but this without the central influence assumed by the stimulus-response theorists. It is true that Tolman (4) has come to believe that certain "types" of learning may not follow the stimulus-stimulus paradigm, or may even depend upon drive-reduction as this concept is used by Hull and his associates. He has held, however, that spatial learning, in particular, is of the cognition variety, and this remaining stronghold of his position has been a continued object of investigation.

Types of Learning

One of the recurrent problems in learning theory is whether all learning reduces to a single type of process, or whether there are different types of learning process occurring simultaneously or in different types of situations (20). Stimulus-response theorists differ among themselves as to whether a need-reduction or a contiguity paradigm is the proper one to embrace all learning, as indicated earlier in this chapter [contrast Hull (1) with Guthrie (6)]. Both of these may be called monistic viewpoints [(20) p. 13] to distinguish them from another and dualistic stimulus-response position represented by Mowrer (21), for example, who argued that the learning of responses mediated by the autonomic nervous system occurs on a contiguity basis, whereas learning of the trial-and-error (skeletal muscle) variety occurs on a Law of Effect basis.

Contrasting with both of these stimulus-response positions is Tolman's pluralistic position, namely, that there are several different types of learning, defined by what is learned and accounted for by somewhat differing sets of laws. While Tolman has indicated [(4) p. 154] that need-reduction may be an operative principle in the learning of "cathexes" and "equivalence beliefs," it is not viewed as important to the other types of learning. There a contiguity principle, in one form or other, continues to be accepted. Tolman's often mentioned acceptance of the need-reduction principle, it should be noted, does not in fact extend to the very class of experiments which were

done in the stimulus-response attack upon his views, viz., experiments of the field-expectancy (latent-learning) variety, for there his statement is [(4) p. 151]:

> . . . I would conclude that motivation conditions must be assumed to play a role in the building up of field expectancies. But this does not mean that I hold that such learning consists in the stamping in of S-R habits by reinforcement. The presence of reinforcement in a particular locus makes that locus a goal which determines what performance will take place but it does not stamp in S-R connections though it probably does give a special vividness to that locus in the total field expectancy.

The principal recent paper bearing upon the general controversy over whether there are different types of learning was published by Birch & Bitterman (22). They are at times critical of the cognition view, but they are definitely against a monistic position. Rather, they argue for the view that in addition to selective (Law of Effect) learning there is also sensory integration (contiguity) learning. To show the present status of the general controversy, somewhat critical attention will be given to this paper, although it will not be possible to deal fairly with all the arguments or evidence.

In the first section of their paper, Birch & Bitterman examine several types of experimental evidence, and indicate what they believe to be the inadequacy of a need-reduction principle of learning in accounting for the data. But such data can be directly accounted for, they believe, by a sensory integration principle. The first question, then, is whether need-reduction or reinforcement theory is in fact inadequate.

There is reason to believe that the claimed inadequacy of reinforcement theory results from a failure by Birch & Bitterman to utilize a portion of this theory which, it is true, is not highly developed in their main reference, Hull's book (1). The theory is available in the publications of Miller [e.g. (23)], however, and a brief reminder of it here will shorten the later discussion. This bit of theory deals with secondary motivation. According to Miller, an animal shocked for several trials in a certain compartment will acquire a fear response to it (pain serving as the unconditioned stimulus, being reduced by cessation of shock, or escape; fear being the innate response to pain; compartment stimuli serving as conditioned stimuli). This fear response, when made, produces strong (acquired drive) stimuli, which can motivate further behavior if the cues again elicit fear. That is, if the animal becomes afraid when placed in the compartment, and the fear-produced stimuli are reduced by any change in the circumstances (as by a response) this drive reduction will be reinforcing to this contiguous response.

As a first example, Birch & Bitterman quote Hilgard (24) concerning the stimulus-response treatment of avoidance learning; Hilgard commented that according to reinforcement theory, literally taken, a rat which runs down an alley, is shocked, and dashes ahead, should on the next occasion do the same thing. Sheffield [(7) p. 288] indicates that while a few rats do learn in this way, more animals learn to slow up or avoid. The latter behavior is what Birch & Bitterman consider damaging to a reinforcement viewpoint,

for it seems to imply that stimulus onset (of the unconditioned stimulus) rather than termination (pain reduction) is the important factor. Avoidance behavior in such a situation is, however, readily accounted for by Miller. When the animal is first shocked, this shock elicits jumping forward, let us say, and also elicits fear, in the presence of compartment cues at that place. On a later trial, the cues at or near this place may elicit the fear response before the animal gets to the grid. If the rat's reaction to fear is to crouch, or slow down, the latter responses will produce avoidance of shock and reduction of fear, and so are learned to the cues. In the presumably rarer cases where the rats continue to run ahead on successive trials, the exact time relations within the learning process and the relative strengths of different response tendencies should provide sufficient explanation [see Gwinn's study (25)].

As another example, Birch & Bitterman [(22) p. 297] call attention to a study by Brogden, Lipman & Culler (26). These investigators extinguished an avoidance reaction in dogs under two conditions. In condition A, occurrence of the conditioned response was followed by thoracic shock. (Leg shock had previously served as the unconditioned stimulus.) That is, extinction was motivated by punishment for making the conditioned response. In condition B, extinction involved only omission of shock. Extinction was much faster in condition A and this, according to Birch & Bitterman, goes counter to what would be predicted from reinforcement theory [see (22) p. 297]:

> A strict application of Hull's interpretation of the role of shock leads to the prediction that extinction should occur more rapidly under condition B, since the termination of thoracic shock which closely follows each tone-flexion conjunction in condition A should constitute a reinforcing state of affairs and thus tend to maintain the response.

We may note two things about this interpretation. First, according to the acquired-drive viewpoint presented above, a fear drive would be learned under shock conditions, and this could be reduced by avoidance behavior. Hence, extinction of a fear-motivated response (condition B) should be a lengthy process, as it commonly is, and as it is found to be in these data: only "by chance" will the animal fail to avoid, thus to learn that the shock is no longer forthcoming. In condition A, the question of time-relations is also involved. Shock to the thorax is known [(27) p. 586] to produce body-twisting and other responses in dogs; assuming the fear drive to be present and operative, it nevertheless should be true that the effect of the thoracic shock would be to elicit responses in competition with flexion, especially responses necessary to maintain body balance during flinching from shock. Such responses are closest in time to shock and the subsequent pain-cessation on those trials in which flexion has not yet extinguished; they also make it probable that, since flexion is less likely, the animal will more quickly experience foot-down-but-no-shock, thus to speed extinction of the fear response. Motivated extinction in this situation should, therefore, be faster according to reinforcement theory.

Several other experiments cited by Birch & Bitterman as critical of the reinforcement viewpoint may be given similar interpretation, if the secondary-motivation extensions are included.

Another main argument for sensory-integration as a separate principle of learning is based by Birch & Bitterman upon the sensory conditioning studies of Brogden (28). These studies are themselves open to a possible reinforcement interpretation, on the grounds that the seemingly neutral pairing of, say, light and tone, antecedent to conditioning involving one of them, may actually provide reinforcement conditions for associating them.[2] Again, intensities of stimuli employed, and time relations between them, may be important factors in these experiments, and they may be brought within the reinforcement framework.

Other arguments of Birch & Bitterman are partly neurological in nature, and will not be considered here. Certainly it is safe to say that the sorts of weaknesses which have been pointed out in their case will not be conclusive in the continuing controversy about monistic and pluralistic approaches to the basic nature of learning.

Latent Learning

The phenomena of latent learning are a persistent source of disagreement between reinforcement and field-cognition theorists. Since latent learning is purportedly learning wherein a response is not accompanied by primary drive reduction or, presumably, secondary reinforcement, acceptance of latent learning as valid would require emendation of reinforcement theory. On the other hand, demonstration that latent learning always involves reinforcement would force the cognition theorist seriously to reconsider his views. The papers published during the year cannot be said to settle the issue.

Tolman & Gleitman [(29) p. 810] grant that "rats do not readily learn the location of one type of goal-object when under another different drive." This statement is made in view of the negative results of a number of earlier studies [for a summary see (20)] in which animals under strong drive continued, when that drive was sated and another strong one introduced, to exhibit the previously correct response. The authors report an experiment in which rats were given equal experience in going to the two very dissimilar end-boxes of a T-maze, being hungry and always finding food in either box. After a period of such training, the animals were shocked individually in one of the end-boxes, which for this purpose had been removed to another room. Two hours later the rats were run on the original, reassembled maze. Of 25 rats, 22 chose the end-box where they had not been shocked.

Performance on test trials is suggested by Tolman & Gleitman to show learning without reinforcement, i.e., the animals had never been shocked following a run to one side or the other in the maze, yet on test trials showed consistent evidence of avoidance. However, the authors themselves note that

[2] As pointed out by Professor Kenneth W. Spence.

the data could probably be explained on the basis of avoidance or fear responses, conditioned to stimuli produced by anticipatory reactions occurring in the food box: on the test trial, the anticipation-produced stimuli would tend to elicit an avoidance response which would negate any tendency to approach the end-box in which the shock had recently occurred.

Gleitman (30), in analyzing factors which might have worked against the appearance of latent learning in some of the earlier experiments, put two hypotheses to test. One was the notion that where rats were run under a certain drive, and on half the trials forced to go to the side of a maze opposite the relevant reward, they were really learning to avoid the irrelevant side because of the emotional disturbances coincident with being forced to go there. The other hypothesis was that, during original training in which animals secure a reward on one side of the maze, the action of a strong drive pertinent to this reward narrows the "cognitive map" of the animal, so that nothing can be learned about a then irrelevant goal object elsewhere. In Gleitman's own experiment, the data offered no support to either hypothesis.

In the papers just discussed, the authors express a strong interest in exploring all variables in the latent-learning type of experiment. The need for this is further made plain by the results of Walker, Knotter & DeValois (31). In their procedure, rats were trained in a single-choice Y-maze, under thirst, with water in both end-boxes and food in one of them. (In different groups, the proportion of free and forced training trials was varied.) Then a test trial was given under hunger. There was a significant latent-learning result. The animals now clearly tended to go to the food side to a greater extent than previously. This tendency was least significant when all training trials had been made by the forced-choice method, a finding which may turn out to be significant in its own right. It was also possible to determine from the data the strength of position preference shown by each animal, and to test a secondary hypothesis that the weaker the position preference, the more likely was the animal to show any acquired food-going preference on the test trial. This hypothesis was substantiated, and the competing habit tendency is thus marked for further investigation. Just why the experiment should produce results opposed to those of Kendler & Mencher (32) and Grice (33), or indeed to those of Walker himself (34), is admittedly not yet clear, but evidently the systematic exploration of some of the suggested variables is now required.

In an effort to avoid interactions between hunger and thirst drives, MacCorquodale & Meehl (35) ran rats in a simple T-maze with both drives sated simultaneously, both "relevant" incentives in one goal box, and nothing in the other. Then, on a test trial, the animals were both hungry and thirsty; they showed a very significant tendency to choose the correct end-box. But there had been a demonstrable preference for the incentive side of the maze during the training trials, even though no incentive was ever consumed. The apparent influence of secondary reinforcement thus indicates that the data can not be considered a crucial test of differences between stimulus-response and field-cognition viewpoints. Nevertheless, in showing the positive

influence of such secondary reinforcement, the experiment is valuable as another indicator of a solution to the latent learning puzzle.

Final mention should be made, in the latent learning group of studies, of Seward & Levy's paper (36) concerning what might be called latent extinction. In their procedure, the experimental group of rats, when extinction began, were placed in the now-empty food box away from the maze proper, in addition to running the usual extinction trials. The control group experienced only a different empty platform, in addition to its extinction trials. Measures of starting time and running time in a straight elevated runway both showed that extinction was more rapid in the experimental group. The results, as Seward & Levy point out, do not provide a crucial test as between the reinforcement and cognition viewpoints, for the behavior of the experimental animals can be explained via the fractional anticipatory goal reaction mechanism developed by Hull and his associates, and equally well via the modification of sign-gestalt expectancies. Whether there is more to latent learning than stimulus-response theory can handle is simply undecided as yet.

Place versus Response Learning

The several studies (37) seeming to show that rats learned to go to a particular place for a food reward more easily than to make a particular turn, have been followed by papers both supporting and questioning this result. The issue is whether or not place learning, which seems indicative of cognition or "map" learning tendencies, or response learning, which is suggestive of specific stimulus-response associations, more nearly fits the facts about animal behavior.

Tolman & Gleitman (38) followed the same general procedure as that in the earlier California studies, but were concerned with the effects of degree of hunger and with the specific effects of visual place cues. Rats were run on a simple T-maze which could be reversed in position so that the rats in one group had always to turn left (right), in order to find food, while the rats in another group had always to turn west (east). Later, all animals were blinded, to test for the importance of visual cues in their maze behavior (since vision is presumptively most important for place learning). The results showed that learning was better in the place groups, but that degree of hunger during learning was not significant for this finding. The place-learning groups were more disturbed by being blinded. This seemed to rule out the possibility that response-learning was slow because it consisted of double-place learning.

Supporting data for the belief that place learning characterizes rat learning were published by Ritchie, Aeschliman & Peirce (39). They point out a difficulty in their earlier statements about place learning (37). They had assumed that the test for place learning is to put an animal at a new starting point for a test trial; they had also assumed that the new location is discriminably different from the old. Therefore, a failure to show place learning on a direct test for it might be due to a failure of such learning to occur, or, equally well, to a lack of discriminability of the two locations. They there-

fore devised an indirect test for place learning. A simple T-maze was designed in which the kinesthetic cues preceding the principal choice reaction could be varied from trial to trial (by varying the turn pattern of an initial path segment). An experimental group of rats was trained in this fashion, a control group ran the same initial segment of the maze but always with the same turn pattern, and another control group ran over the usual straight initial segment. All groups were then tested with a new starting point. The experimental group then showed a clear place response, and the two control groups showed chance tendencies in their choices of turn. In interpreting this result, the authors argue that the procedure of preventing consistent kinesthetic cues from developing permits place learning to show itself in the experimental group. By contrast, the consistent kinesthetic cues permitted in their control group, and in other experiments (40), lead to the development of response-choice tendencies which counteract any place-choice tendencies. They therefore believe it definitely possible that place learning characterizes simple T-maze learning, especially in its early stages.

Results which contradict the empirical findings of California workers have been reported by Kendler & Mencher (41). Such results are also reported by Blodgett, McCutchan & Mathews (42). Hence, a few comments will be made about this study because it is the kind of which Ritchie *et al.* were directly critical. Blodgett and his co-workers employed a simple T-maze which could be placed in any one of four positions. By choice of these placements, the investigators could distinguish between direction, response, and place choices, and several combinations of these. It is their conclusion that direction-learning was fastest, response-learning next, and place-learning poorest. These findings will now have to be interpreted in light of the findings reported above concerning the role of kinesthetic cues in these types of learning situation.

An experiment by Thompson & Thompson (43) bears on a factor which may be crucial in several of the place response learning studies. These authors noted that the learning trials, in the work of Tolman and his associates (37), were relatively massed. It therefore seemed possible that alternation of choices on successive trials would occur because of failure to allow time for reactive inhibition (1) to dissipate between trials. This would act to favor the apparent learning of a place preference (where the animal starts from different directions, and thus must vary his turn-choices if he is to learn), and work against the learning of, say, a left-going response. Thompson & Thompson therefore introduced the variable of distribution of practice into the general California design. They were able to show that while place learning was somewhat (although not significantly) superior to response learning under massed trials, distributed trials produced significantly faster response learning. Just how many of the California data may be given such an interpretation is a matter yet to be decided.

Motivation and Secondary Reinforcement

As both an instigator and a learnable feature of behavior, motivation has received special attention from students of the learning process in recent

years. In the latent learning problem, especially, the typical experimental design is one which assumes that a rat can discriminate between being hungry and being thirsty. Were this not a defensible assumption, many of the supposedly critical tests of cognition and reinforcement theories would be meaningless. The various subtleties of secondary reinforcement, furthermore, are likely to turn out to be highly important in explaining some of the as yet puzzling data of latent learning, discrimination learning, etc. Therefore, three brief discussions will record some of the work recently done on motivational problems by students of learning.

Drive discrimination.—Kendler (44), on the basis of an experiment in which rats learned a T-maze under simultaneous hunger and thirst drives, had showed that the animals could respond appropriately when later tested either hungry or thirsty. Two hypotheses about the nature of the drive-response-association process were presented. The first, challenging to the Hull viewpoint (1), was that associations were formed only between the response and the relevant (reduced) drive stimuli, rather than all stimuli present when the response was made. The other hypothesis was that anticipatory goal reactions to food and water, developed during the earlier life of the animals, provided a differential at the choice point. To test this notion, Kendler (45) reared rats on a liquid diet, then subjected them to approximately the same conditions as in his first experiment. The data were not clean-cut, but they suggested that animals thus reared were no different in learning to go left or right on the basis of a single drive than were control animals, normally reared. Opportunity to build up differential anticipatory goal reactions (to food and water) in earlier life, and therefore to utilize them in learning within the experiment, thus does not seem significant; so the selective-association principle remains a possibility, according to Kendler.

Amsel (46), however, takes issue with Kendler. In his experiment, rats were either hungry or thirsty during training trials; they were driven to run a simple T-maze by anxiety based on prior (but now absent) electric shock. Which turn would permit escape from this anxiety depended upon which irrelevant drive was operating. The learning was slow, but some individual animals did show definite signs of learning. This, Amsel believes, casts doubt on the selective-association hypothesis, for in his experiment irrelevant drive stimuli (hunger, thirst), while not themselves reduced, evidently became associated with making the proper turn to reduce anxiety. A further group of animals was trained under identical conditions, save for the giving of a small amount of reward relevant to the drive of hunger or thirst. This group learned quickly, thus reinforcing the hypothesis that differential anticipatory goal responses are important in the behavior observed by Kendler.

Winnick (47) arrived at a comparable position as a consequence of an experiment in which rats learned to press a metal panel in order to turn off a bright light over the cage. One group of rats was hungry, another sated for food, during acquisition of the avoidance response (and neither was rewarded with food during this training). The groups were appropriately subdivided as to motivation during extinction. While considerable variability in activity level was produced by hunger-level variation during extinc-

"oddity principle") and, also, gradually develop a set to transfer this learning to new stimulus dimensions. Harlow (57) has also published a report in which his emphasis is not only upon the gradually emerging ability of monkeys to make one-trial solutions to discrimination problems, but also upon some of the underlying variables which influence the acquisition of this ability. He was able to identify several different kinds of errors, and to show that these various tendencies changed at different rates or were of varying degrees of importance in different phases of the acquisition of set. These analyses bear on the controversy between the continuity and noncontinuity theorists [see Melton (20) for a treatment of this issue], and show further that stimulus-response analysis is of great aid in understanding the details of how seemingly noncontinuous learning comes about.

Central to many of the issues discussed thus far is the question of what role is played by perception in typical learning experiments. The tendency of most stimulus-response theorists is to regard perception as just another category of response, whereas a much more prominent function is assigned to it by the cognition theorists. In fact, what has been called sign-gestalt learning might just as well be called perceptual learning, or the learning of percepts, of particular kinds. It can be argued that there is imbalance in both emphases, and that eventually it will be necessary to regard perception not only as response, but also (through response-produced cues and/or sets) as providing "behavioral steer."

An example of work which merits recognition, not only because it makes a beginning on problems of perceptual learning, but because it utilizes stimulus-response concepts in so doing, is that by Lawrence. He has been concerned with the acquired distinctiveness of cues, and has used the transfer type of experiment to measure the influence of this factor upon rat behavior. In his first study (58), the first stage of the experiment required animals to learn a simultaneous discrimination, i.e., one where cues to alternative choices were present at the choice point. One cue dimension indicated the correct choice consistently, and another was arranged to be always present but nonrelevant (randomly paired with the correct-choice cues). In the second stage of the experiment, the animals were presented with a successive discrimination problem, in which the cues were to be found in end-boxes of a simple T-maze. During this training, the previously relevant cue might still be relevant, or the nonrelevant one now be relevant, or neither (in the control groups). The results showed that familiarity with the relevant cue produced faster learning in the transfer stage of the experiment.

In the second study (59), Lawrence reversed the stages of the experiment described above, so that the successive-cue discrimination was learned first, the simultaneous-cue problem second. In the first stage, one cue was consistently relevant, one randomly so. In the second stage, the animals could use either or both sets of cues, and could be tested with opposed cues to determine which actually they were using. Results indicated that learning was faster to a cue which had become preferred in previous learning. When, at a later stage, relearning to only a single cue was measured, the preferred cue

was productive of faster improvement, and, notably, a final reversal of cues was learned more efficiently when the reversal was away from the preferred cue.

It is probable, judging by Lawrence's results, that discrimination learning must be a two-part process: there is a change in the perceptual characteristics of the stimulus (e.g., familiarity, preference for it); also, there is association of it with responses relevant to the problem facing the animal. Since Lawrence is able to handle these proposals in the stimulus-response framework, his papers may well aid in resolving the controversy over whether there is more than one type of learning process (see the discussion of the Birch & Bitterman paper above).

Partial Reinforcement

Studies of partial reinforcement have been involved in the general disagreement between cognition and reinforcement theorists. They have most often been cited as supporting the cognition viewpoint, on the ground that the learner, during randomly unreinforced trials [as in conditioning of the psychogalvanic response with occasional omission of shock (60)], is learning an expectancy that the unconditioned stimulus will sometimes occur, sometimes not, but is possible on any trial. He therefore performs nearly as well as the learner who receives the unconditioned stimulus on every trial. In addition, extinction is slower than for the 100 per cent learner, because the expectancy of the partial reinforcement is what must be extinguished, and the mere absence of reinforcement on a few trials will not change this expectancy much. Reinforcement theorists have utilized the notion of overlap of unrewarded-trial and rewarded-trial effects (2), and of secondary reinforcement during unreinforced trials (61), to account for the "normal" rate of learning under partial reinforcement, but have not had a detailed explanation of variations in extinction rate as these relate to the proportion-of-reinforcement variable. A major review on partial reinforcement has been published by Jenkins & Stanley (62), who conclude that such a stimulus-response interpretation is now possible.

Sheffield (63) has reported a thorough study which supported their conclusion. Her starting point was the hypothesis that extinction necessarily involves different cues from those employed during learning. Omitting reinforcement, by definition, changes the stimulus context, so that the cues to response during extinction fall somewhere out on the wing of a generalization gradient centered on the training stimulus pattern. Decrement in response is thus to be expected.

Sheffield used a straight runway, measuring response time of rats running to a food reward. The reward was given on all trials for four groups of rats, 50 per cent of the trials for another four groups. Of each set of four groups, two learned by massed trials, two by spaced. One of each of the latter set of groups was extinguished by massed trials, the other by spaced. Sheffield's theory applies as follows. The reward-stimulus pattern during training includes, for 100 per cent reinforcement and massed trials, not only food but

also, briefly, such things as particles of food remaining in the mouth, continuing smell of food, temporary relaxation, etc., and these may be present also at the start of the next trial. For 50 per cent reinforcement and massed trials, the animals are conditioned not only to the just described pattern, but also (on trials following unreinforced ones, especially) to the kinds of cues which are later going to be present during extinction trials, i.e., lack of food particles in the mouth, possibly a state of frustration, etc. When extinction begins, then, omission of the food reward should produce a relatively greater change in the stimulus situation for animals in the 100 per cent groups than for animals in the 50 per cent groups, so that extinction should be more rapid for the former. Sheffield's data confirm this expectation.

In addition, according to Sheffield's hypothesis, distributing the learning trials should make the trial-to-trial stimulus pattern more similar for 100 per cent and 50 per cent reinforcement groups, because the inter-trial interval should tend to remove any differential effect of food particles remaining in the mouth, etc. Therefore, a shift to extinction conditions is now not so drastic a change for the 100 per cent distributed group, and it should extinguish little more rapidly than the 50 per cent distributed group. Sheffield found the difference between extinction curves for the spaced and distributed (acquisition) conditions to be insignificant.

It should be noted that Sheffield's results are possibly equally well treated by either a need-reduction or a contiguity theory of reinforcement. In either case, data formerly not yielding to such theorizing now seem to do so, at Sheffield's hands, and it remains to be seen whether the cognition theory can be applied to her data without some revision, particularly with regard to the operation of distribution of practice. It also remains to be seen whether the data on extinction of human conditioned responses acquired under partial reinforcement (60) will yield to Sheffield's interpretation, and here the point may be that some of the data not previously explained thus have not been fully understood; re-analysis may show that a stimulus-response interpretation is valid (62). In passing, it should be noted that Jenkins & Rigby (64) have stressed the consequences of non-reinforced (acquisition) trials as critical to the whole partial reinforcement question.

The Nature of Generalization

As indicated in last year's review (20), the stimulus response theorists, e.g., Hull (65), and the cognition theorists, Lashley & Wade in particular (66), have not been able to agree on the basic nature of generalization, or even on the empirical findings concerning it (67). Since generalization is a major factor in transfer of learning, any learning theory with pretensions to completeness must of necessity include views about the nature of it.

Essentially, it has been assumed by the stimulus response theorists (65) that excitatory and inhibitory tendencies are built up to any particular relevant stimulus (one associated with the response-reward pattern), that these are specific to this stimulus, and that at the time they develop, a functional effectiveness of all similar stimuli also develops, depending upon

just how similar these are to the training stimulus. Discrimination between two or more stimuli is thus a matter of their relative net (algebraically summed) excitatory strengths. Lashley & Wade (66), exemplifying the opposing view, believe that response tendencies are not built up to specific stimuli, but rather occur as a consequence of comparison and discrimination. They further believe that generalization has its (temporal) locus in the moment when a new stimulus is introduced, rather than in the original learning.

Only a few papers bore on this problem during the past year. Grice (68) trained rats in a visual-size discrimination problem, for one group presenting the stimuli simultaneously, for another, successively. Later, each group was shifted to the opposite presentation condition. There was no difference in the rate at which discrimination developed, and both groups responded appropriately when shifted. The careful design of Grice's study lends strength to his statement that no act of comparison seems to have been involved in the learning. A comparable result was arrived at by Webb (69), with a different type of experimental design.

Seemingly opposed evidence is presented by Razran (70, 71). He employed his salivary conditioning technique with college-student subjects, words being used as training (and test) stimuli. His data tend to support what may be termed the "subsequent testing" hypothesis, i.e., that the direction of the generalization process is from the test word to the trace of the conditioned stimulus. This line of thinking is comparable to that of Lashley & Wade (see above).

Verbal Learning and Retention

Studies of verbal learning and retention tend not to be concerned directly with theoretical concepts of the order thus far stressed in this review. Therefore they can not readily be organized into the pattern of discussion of the preceding sections. Somewhat arbitrarily, the author has chosen to group certain important studies under a heading which is not analogous to previous headings.

One of the principal problems in the area of rote learning and memory has been the explanation of reminiscence (improvement in memory during rest, without further practice), and of the related improvement of learning-rates via distribution of practice. One of the most widely cited explanations was based in large part on the differential forgetting hypothesis (72). Error tendencies were supposed to weaken more rapidly than correct-response tendencies during any rest interval, so that interspersed rests would be beneficial to performance of correct responses.

The remote associations which appear in serial learning are error tendencies, and so should be forgotten more rapidly than correct-response tendencies, according to the differential-forgetting hypothesis. Wilson (73), however, has shown that this is not the case, and his methodology was such as to give high credence to his results.

In his earlier work, Hovland (74) had found that distribution of practice produced little benefit in the learning of paired associates. That finding

seemed to argue that remote associations, which are reduced or precluded by the paired-associates technique, must have been important in the advantage which distribution gave in serial learning, where remote associations are prominent. In a recent paper, however, Hovland (75) reports that when the rate of presentation of pairs was increased, so that the response-rate in paired associate learning was comparable to that in his earlier studies using serial learning, distribution of practice was beneficial. This reopens the possibility that benefits from distribution are due to recovery from fatigue or other factors producing response decrement during continuous practice. Another possible explanation of the benefits of distribution, that rest permits "stimulus induced maturation" to occur, was tested for serial learning of nonsense syllables by Wright & Taylor (76), and found not to accord with their data.

In studies of the effects of a rest interval upon retention, certain variables are always operative at or just before the measurement of retention. One of these, usually disregarded, is the factor of "warming-up." Irion (77) first required his subjects, at the time they returned to the laboratory for measures of recall and relearning, to name colors from the memory drum (without learning them). The effect was a significant reduction in the measured amount of forgetting. Another variable is the amount of response time allowed the subject when he attempts to recall. Underwood (78) found, in a standard retroactive inhibition design, that when interpolated learning had been carried to a relatively high level, compared to the original learning, the increase in recall time reduced the measured amount of inhibition at recall of the original list.

Because of its analogical relationship to certain clinical hypotheses, a study by Williams (79) deserves notice. Certain subjects learned paired-associate lists containing some food words, and other subjects learned lists containing some words indicating aggression. But subjects made hungry, or subjects made aggressive by experimentally induced frustration, did not show any selective influence when they recalled the particular lists they had learned. As Williams indicates, this may not be conclusive evidence against the general hypothesis that needs influence memory, for the "laboratory set" of the subject may have provided a restriction which prevented such differentials from showing themselves.

An idea of possible general importance to rote learning theory is found in Gagné's paper (80) on the effects of sequence of presentation of similar and dissimilar items. He considers the possibility that rate of learning depends upon opportunities for differential reinforcement within the list. Certain list structures (e.g., similar items grouped together), create opportunity for overt errors of confusion. Such errors are usually thought to be indicative of difficulty in learning. Gagné suggests, however, that they may actually be conducive to faster mastery of the total list, for in making errors, especially early in learning, the learner can be differentially reinforced for these and for correct responses. The relationship of this hypothesis to the work of Gibson (81) is made apparent by Gagné.

Quite clearly, in the present author's opinion, human rote learning will be understood better only when a genuine theoretical framework has been developed for dealing with it. General stimulus-reponse theory, as exemplified in earlier sections of this chapter, certainly deserves thorough trial in the rote learning field, but as yet has had it only piecemeal.

Motor Learning and Retention

Even more than the verbal learning area, motor learning research has suffered from a lack of integrating theory. In recent years, the writings of Hull (1) have influenced some motor learning studies, but the motor-performance theory of Ammons (82) is apparently more readily applicable, especially to rotary-pursuit learning. A theoretical schema for key-pressing reactions, but applying equally well to nonmotor situations, has been published by Gagné, Baker & Foster (83).

Distribution of practice.—The question of whether the effects of work-period length and rest-period length interact (in the statistical sense) led Kimble & Bilodeau (84) to investigate these two variables in a study of manipulative skill. Their conclusion was that the two variables are independent, which means that for predictions of the effects of various work-rest cycles it is possible simply to summate their effects. Kimble & Bilodeau also indicated that the effects of the work-length variable were the same early and late in learning, whereas the advantages of a longer rest period increased with the stage of practice. These findings were confirmed by Kimble (85). Somewhat contrasting results were secured by Cook & Hilgard (86), whose subjects practiced on the rotary pursuit task.

Reminiscence.—As the study by Hovland (72) shows, reminiscence in verbal learning has usually been thought to involve differential forgetting rates for error and correct-response tendencies, but there has also been the possibility that verbal reminiscence involves recovery from something like fatigue. In motor reminiscence phenomena, by contrast with verbal learning, explanatory devices have stressed work-decrement factors.

Two of Hull's concepts (1), reactive inhibition and conditioned inhibition, were involved in the formulation of a study by Kimble (87), which employed an alphabet-printing task. Reactive inhibition, which is assumed to dissipate with rest and therefore to be the principal cause of reminiscence, was shown by Kimble to decrease as the inter-trial interval during learning increased in length; reminiscence, conversely, was greatest in groups having nearly or literally continuous pre-rest practice. Because only the group having literally continuous practice before rest showed any "permanent" decrement after rest, it is Kimble's argument that conditioned inhibition (which is a habit) does not develop except where reactive inhibition is great enough that its reduction can be reinforcing. Kimble also believes that after enough reactive inhibition develops, the learner automatically begins a partial resting activity of some kind. This keeps reactive inhibition from increasing indefinitely, and provides ("fatigue") drive reduction to condition the learner not to exert himself unduly, thus to show less reminiscence after rest.

A later study by Kimble (88), employing the rotary-pursuit task, distinguishes between the effects of the negative motivation of reactive inhibition and the effects of competition between learners. He was able to show that highly motivated learners (those who were inferior in man-to-man competition and presumably would therefore try harder) showed more reminiscence, probably because they would tolerate more reactive inhibition, and therefore would recover relatively more during a rest interval.

Amount of reminiscence in rotary pursuit learning has been related to certain empirically defined variables by Irion (89). He found that as amount of pre-rest practice increases, amount of reminiscence increases and then decreases; that amount of warm-up effect after rest increases with amount of pre-rest practice and also with increasing length of rest interval; that amount of reminiscence increased as length of rest interval increases, up to the five-minute length of rest, which was the maximum used by him. In general, Irion's results confirm those of Ammons (82, 90).

Summary

The year ends with no sudden change in the status of major problems in the field of learning. It may safely be said that the weight of evidence, in the controversy between stimulus-response theory and cognition theory, is even more heavily on the side of stimulus-response theory than it was a year ago. Yet certain stubborn pro-cognition facts remain.

Analyses of the role of secondary motivation in general, and in the latent-learning experiment in particular, may bring the two general types of theory in sharper contrast, or may bring them together. Similarly, perceptual learning may be an area in which broad disagreements between the two general types of theory may be reduced, and it is noteworthy that some stimulus-response theorists are beginning to work on perceptual learning qua acquisition of percepts, rather than as a sideline.

Verbal learning and retention, and motor learning and retention, have developed no generally accepted theoretical framework. Such theory as exists or may be expected to develop further, is related to the general learning theory which is the main concern of investigators in the field. The lines of relationship, however, are clear in only a few studies.

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VISION

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A noticeable difference between this and the last three annual reviews of vision (1, 2, 3) is that this one cites more books and reviews of vision than did the earlier ones. Book writing is a long-term job and this probably means that visual scientists have finally recovered from restrictions in activity imposed by the war. Several books and reviews are mentioned later; those in this section are of more general interest.

Davson's text (4) is an intermediate level one for physiologists and medical students; psychologists will find its coverage of physiological material good, of psychophysical and perceptual material only fair. Prince's book (5) is supposed to be a sort of comparative psychophysiology of vision. Although there is much of interest in it, it is a pot-pourri of miscellaneous and incidental information. Linksz has published the first of a three-volume set on the *Physiology of the Eye* (6). The first volume is about optics and it should be a highly useful text because it was written by a nonphysicist for students who are not physicists. The research monograph by Gesell, Ilg & Bullis (7) on the development of human vision is disappointing. It is highly verbal and contains virtually no quantitative data on the dozens of highly intriguing problems that need to be explored in this area. Finally, Birren (8) has published another enthusiastic and uncritical account of the alleged effects of color on personality, attitudes, and moods.

Basic Visual Functions

Dark adaptation.—The visual literature contains many studies of the area-intensity relationship for rod and cone vision and these would have enabled one to predict fairly well in advance the results of a study by Wolf & Zigler (9). These investigators studied dark-adaptation functions with four sizes of test light under two essentially different conditions: (*a*) the subject was required to detect only the presence of a light, and (*b*) he was required to detect the presence of parallel lines in the test field. As one might have expected, cone and rod thresholds are consistently lower for test fields of large area than for those of small area. This appears to surprise the authors and they spend some time commenting on the absence of a clearly defined boundary between photopic and scotopic vision. Actually, Wald (10) showed in 1938 that such relationships are consistent with photochemical theory and with statistical predictions based essentially on assumed random fluctuations among individual receptor thresholds.

Wolf & Zigler also found that dark adaptation curves measured with a criterion of form perception are higher than those with a criterion of light perception. This is also predictable from other studies, but we have not had

very complete data on this point. The curves dealing with this comparison in the Wolf-Zigler article are useful in re-emphasizing the differences between dark adaptation and night vision.

Chapanis, Rouse & Schachter (11) have examined experimentally Russian claims of large effects of inter-sensory stimulation on dark adaptation and night vision. These authors find nothing to substantiate these claims.

It is well known that dark adaptation deteriorates with old age, but new evidence by Birrén & Shock (12) shows that it is the level, and not the rate, of dark adaptation which is affected. The reason why dark adaptation declines in the elderly is elusive. Ophthalmological findings on 109 male subjects, aged 40 to 83, show that the increase in light threshold is so intimately associated with macular and lenticular degeneration that it is hard to determine the effect of age per se [Birren, Bick & Yiengst (13)]. As nearly as these investigators can tell, however, the explanation does not lie in age changes in the availability of vitamin A in the blood. There was no change in thresholds following the administration of massive doses of vitamin A over 38 days. McDonald (14) also reports negative results on vitamin A administered to a younger group of subjects over eight weeks.

Grant & Mote (15) have studied the effects of brief flashes of light on dark adaptation and show that the effects are much less than one might at first suppose. According to Thomson (16), however, variations in the light environment of the subject for some time preceding dark-adaptation will show up in the dark-adaptation curve. Ordinary amounts of light-adaptation given in the laboratory prior to a dark-adaptation run are not sufficient to mask out the effects of previous light history.

Subjective brightness scale.—Psychologists have long known that subjective brightness cannot be identified with physical intensity, or luminance, but no one had ever done a careful job of finding out how the two functions are related. Using a fractionation technique, Hanes (17) was able to construct a scale of subjective brightness over a range of luminance from 0.0001 to 500 millilamberts. The subjective brightness scale so constructed agrees closely with the curve obtained by integrating DL's (difference limens). It is essentially uninfluenced by changes in hue and stimulus duration, and it is internally consistent as shown by the fact that similar results are obtained with fractional and multiple estimates of brightness (18).

Visual acuity.—Binocular visual acuity is almost invariably better than the acuity of either eye. Why? A part of the difference, according to a study by Horowitz (19), can be traced to changes in pupil size which in turn are known to affect acuity by changing the size of the blur circles on the retina. When monocular acuity is tested, one eye is closed or covered. This causes a reflex dilation of the pupil in the other eye and thereby a decrease in acuity. In short, artificial pupils reduce the monocular-binocular difference.

Another part of the answer is that when one eye is covered, there is a difference in the intensity of stimulation for the two eyes with the possibility of some kind of interaction. If the nontested eye views a blank field illuminated to the same intensity as the field viewed by the other eye, the monoc-

ular-binocular difference is reduced still further. Exactly why this happens is a matter for speculation at the moment. Pupil changes and binocular differences in intensity of retinal illumination account for much of the monocular-binocular difference in acuity. They do not account for all of it, however, and we must wait for further research to complete the story.

Of the various acuities, vernier acuity has been largely neglected in the experimental literature. Our knowledge of this function has now been rounded out by Baker's study (20). This investigator studied vernier acuity as a function of illuminance (over somewhat more than 5 log units), two durations of exposure (unlimited, and exposures of 0.019 second), and color of illuminance. She found that this kind of acuity, like others, shows a characteristic rise with increase in amount of light. The functions reach a maximum at intensities above 1,000 photons, at which values the maximum acuity is equal to 7.6, corresponding to a discriminable separation of 0.132 min. of arc (normal Snellen acuity is usually taken to be 1.00). Brief exposures displace the curves downwards. Acuity is highest under red light, and progressively less under yellow, white, and blue light. When the difficulty of focusing under blue light is corrected with a lens, acuity under blue light is best. This order of acuities under the different colors of lights is different from what has been found in other acuity studies [see (21), for example] and there seems to be no way of accounting for the discrepancy at the moment.

For years we have been taught to believe that visual acuity is a matter of brightness discrimination. But now a nice experiment by Senders (22) shows that in at least one respect acuity and brightness functions are not the same. When interrupted light is used, visual acuity is much better than you would predict from the intensity of the light calculated from the Talbot-Plateau law. Although clinical data usually constitute a shaky kind of evidence for visual theory, there is confirmation of this finding in a study by Miles (23). Miles found that in amblyopic patients there is no correlation between visual acuity and CFF (critical flicker frequency)—also a matter of brightness discrimination according to traditional photochemical theory.

A couple of years ago, Higgins & Stultz (24) showed that visual acuity measured with a parallel-line test object is appreciably higher when the lines pass through the visual field either horizontally or vertically than when they pass through the visual field at an angle of either 45 or 135 degrees with the horizontal. Brown (25) recently attempted to explain this finding in terms of preferential directions of eye movements due to the arrangement of the external eye muscles and the damping of eye movements by viscous friction. Hardly had Brown's hypothesis been published when another article by Higgins & Stultz (26) refuted his explanation. With exposures of less than 1 msec., much too short for any eye movements, the differential visual acuity still shows up. This leaves us right where we started—with no explanation for this interesting effect.

Color vision.—The publication of Volume III of the *Documenta Ophthalmologica* (27) was probably the most important single contribution to the

literature on color vision to appear in the last year. This volume contains most of the papers which were read at the Conference on Colour Vision held at Cambridge, England, between July 28th and August 2nd, 1947. Contributors to this volume and the titles of their papers are: W. D. Wright, "The present status of the trichromatic theory"; S. Polyak, "Retinal structure and colour vision"; W. E. Le Gros Clark, "The laminar pattern of the lateral geniculate nucleus considered in relation to colour vision"; R. Granit, "The analysis of retinal elements by the micro-electrode technique"; G. Wald, "The photochemistry of vision"; W. S. Stiles, "Increment thresholds and the mechanisms of colour vision"; H. Hartridge, "The polychromatic theory"; E. N. Willmer, "Colour vision in the central fovea"; D. L. MacAdam, "Colour discrimination and the influence of colour contrast on acuity"; J. G. Holmes, "Colour recognition of very small light sources"; D. B. Judd, "Current views on colour blindness"; S. Hecht, "Brightness, visual acuity and colour blindness"; F. H. G. Pitt, "Some aspects of anomalous vision"; S. Krauss, "Phenomena observed in veiled colours"; A. Ivanoff, "Chromatic aberration of the eye"; and M. A. Bouman and H. A. van der Velden, "The two-quanta explanation of the dependence of the threshold values on the visual angle and the time of observation."

Since this is a collection of papers, it is not as well-integrated as a textbook would be. The subject matter with which the papers deal, however, is such a restricted one that there is good continuity throughout the volume. The importance of this book is that it is a collection of papers by the world's foremost authorities on color vision. Some have chosen to present summaries of the literature, some have been mainly theoretical, and some have discussed current or recent researches, reports of which can be found only in widely separated journals. Several of the papers in this book are referred to in more detail elsewhere in this review.

The psychophysical dimensions of the color space generated by the I.C.I. trichromatic coordinate system have already been explored in several investigations [see (28), for example]. In every case, these have dealt with the perceptibility of colors of equal intensity but varying in hue and saturation. Brown & MacAdam (29) have now extended this kind of work into the brightness dimension. They studied thresholds of discrimination for colors which varied simultaneously in intensity, hue, and saturation from 39 selected reference colors. The results form a series of ellipsoids which are very nearly symmetrical above and below the line of constant chromaticity. This is a good piece of work but the authors have treated their data with such high-powered mathematical methods that few psychologists will be able to use them. This is unfortunate, since the results of research are of no value unless they are communicated. A little more effort put into the preparation of this article would have made it understandable to many more people.

Color adaptation.—In his work on color adaptation, Cohen (30) found that the brightness of a test light increases with continued exposure. This is contrary to most earlier findings and was somewhat of a puzzler until

Jameson & Hurvich (31) came up with a solution. In studies of this sort, variations in brightness of the test light are customarily measured by equating to it a comparison field of variable intensity in the other eye or in some other retinal location. Changes in the intensity of the comparison field are then used as measures of the adaptive changes in the test area. Unfortunately, the comparison field itself undergoes adaptation at the same time that the test field is being viewed. As a result, Jameson & Hurvich showed that the brightness of the test field increases (as Cohen found) if the comparison eye is adapted to a higher luminance level than the test field. If, however, the comparison eye is adapted to the same luminance level as the test field, there is no apparent change in the brightness of the test light during color adaptation. Finally, if the comparison eye is adapted to a luminance lower than the test field, the brightness of the test light decreases with time (as many earlier investigators found). What really happens during color adaptation constitutes a good problem for research, if anyone can discover how to get out of this box!

The classification of color-vision defects.—Color vision experts seem generally agreed that to understand normal color vision, we need to understand color vision defects. Recent research in this area has not been very helpful. Sloan compared a simple dichroic-filter anomaloscope with a Nagel anomaloscope and found substantial agreement between the classifications made on the two instruments (32). More interesting from a theoretical standpoint is her description of the settings made on the Nagel instrument by 103 color-deficient subjects. Thirty-one subjects were able to match red with yellow and green with yellow, and so were classified as dichromats. How this could happen is somewhat of a paradox in view of the careful measurements published by Hecht & Shlaer in 1936 (33) on the wavelength discrimination and saturation discrimination of three dichromats. According to the Hecht-Shlaer data, some dichromats at least should be able to discriminate spectral yellow from green and from red. In any event, Sloan's 31 dichromats fall into two distinct, nonoverlapping groups (protanopes and deuteranopes) when they are ranked with respect to the intensities of yellow selected to match red and green.

Fifty-six of Sloan's color-deficient subjects were not able to match pure red and yellow or pure green and yellow. They could, like the normal subject, match yellow to one particular mixture of red and green, but they differed from the normal in the proportions of red and green which they required. Nine subjects gave green-red quotients ranging from 0.046 to 0.346 and the remaining 47 ranged from 1.90 to 8.05. Sixteen normal subjects had quotients ranging from 0.85 to 1.13. To complete the picture, four color-deficient subjects had normal quotients and 12 gave readings which seemed transitional between those of dichromats and anomalous trichromats. Actually, Sloan gives evidence to suggest that many, if not all, of the 12 are dichromats who gave atypical responses because differences in saturation were mistaken for differences in hue.

These results are consistent with the findings of Pickford (34) who con-

cluded that red-green defectives fall into five groups which are "statistically distinct from the 'normal' group and from each other." This is a revolutionary notion because it has been customary for color-vision experts to say that anomalous trichromats are intermediate between individuals with normal color vision and those with dichromatic vision. The usual statement adds that anomalous trichromats form a continuum between the normal and dichromat. Sloan and Pickford, however, would seem to argue that anomalous trichromats form two special classes of color-vision defectives (protanomalous and deuteranomalous), distinct both from the normal and dichromats. If true, this would have some important implications for color theory.

These ideas, however, not only run counter to old-established viewpoints, but they also conflict with other research published almost simultaneously. Farnsworth (35), for example, studied the chromaticity confusions made by 19 color-deficient subjects and concluded that there is "a continuum between anomalous trichromacy and dichromacy within each type." In an article in the *Documenta Ophthalmologica* (27), Pitt stated "it is difficult to say with any degree of certainty when an extreme case of protanomaly becomes protanopia, and from the argument outlined in this paper it may be that true protanopic vision, i.e., dichromatic, does not theoretically exist but exists only because the R′ response is so small that it is not ordinarily measurable." Finally, we may recall the set of hue-discrimination curves for 11 protanomalous individuals published in Wright's book just a few years ago (36). These curves show an almost perfect continuum from the normal to the protanopic function.

In other respects, too, there are conflicting data on color-vision defects. In an article in the *Documenta Ophthalmologica*, Judd points out that protanomalous individuals are not intermediate between the normal and protanopes with respect to visibility functions. Thus, "the luminosity function of protanomalous observers of all degrees is deficient at the long-wave end nearly, if not exactly, like that of the protanope." Yet Chapanis & Halsey (37) reported that in a random sample of 47 color-deficient individuals, photopic thresholds to red light form a continuous distribution rather than two distinct distributions.

What is the reason for all these conflicting data? Even this is not clear. One point which might need stress is that guarantees of random sampling are crucial for arguments like these. Many studies are done on only a few color-deviant observers—they have to be because of the amount of time required for careful laboratory investigations. When I see data on a few protanomalous individuals, for example, I always wonder exactly how they were selected. Did the experimenter, mindful of the necessity for getting clean data, perhaps inadvertently pick those individuals who were "good" cases, that is, fit a type? Wright frankly admits that his cases were selected. This is all right so long as we remember to be careful when we use these data in arguments about "types" of color deficiency. The question of "types" of color deficiency, and whether color vision defects are continuous or dis-

continuous, is important not only for color theory but also for the practical problems of testing and selection. Unfortunately, these questions may never be settled to everyone's satisfaction until several thousand unselected individuals are carefully studied with respect to several quantitative aspects of their color vision.

Color-vision defects.—A disturbing group of papers was published last year by Pronko, Bowles, and their students, Allen, Snyder & Synolds. In two papers (38, 39) these authors, in different combinations, reported results obtained on the Dvorine Color Perception Testing Charts which they administered to preschool children, kindergarten children, and children in the first three grades of school. In all, 157 boys and 162 girls were tested but—and this is important for the proper evaluation of their findings—the largest single group was made up of third-graders who numbered 35 boys and 35 girls. The results show that (*a*) very young children earn much lower scores than older children, (*b*) within any age or grade group there is no significant difference between the average number of plates read or traced correctly by the boys and girls, and (*c*) similar color-vision plates are not equally difficult. In a third paper (40), another combination of these authors reported no significant difference between the average number of plates read correctly by 64 adult males and 56 adult females.

There is nothing wrong with these findings; it is the conclusions drawn from them which are at fault. The first result is, in fact, confirmed by Chapanis (41) who found in a larger sample of 574 subjects that young children do not do as well as adults on five pseudo-isochromatic tests. That is what you should expect; after all, young children do not do as well as adults on almost any test. And, although Pronko *et al.* are apparently unaware of it, there are many references in the literature to factors, other than color discrimination, which affect performance on pseudo-isochromatic tests [see (42), for example]. At any rate, Pronko and his group conclude from their studies that there is no such thing as inherited "color blindness," that color vision is all a matter of development and learning, and that there is no difference between the sexes in color vision. This interpretation appears to be the result of (*a*) a naive trust in results obtained on a test with questionable validity, and (*b*) ignorance of an important fact about statistics, namely, that it is much harder to prove no difference between two groups that it is to prove a statistically significant difference between two groups. Since dichromatism occurs with an incidence of about 4 per cent among men and about 0.5 per cent among women, we should expect to find about two or three men out of 64, and no women out of 56, with widely deviant scores. This is exactly what Pronko *et al.* turned up in their last study on adults. With only two deviant scores out of 64, it is not at all surprising that the mean score for the men is not significantly lower than the mean score for women. The unfortunate thing is that years from now unsophisticated graduate students will still be wasting their time on these papers wondering if there might be something to it.

The views about color defects which Pronko and his group would have

us believe form the basis of the doctrine accepted by many optometrists that color-deficient individuals can be "cured," "rehabilitated," or "reconditioned." It is too much to hope that we shall ever be free from such uncritical ideas so long as people persist in ignoring the literature. In the meantime, two minor studies have provided additional evidence about the falsity of such a notion. Chapanis (43) tested three men, two of whom had been "cured" by optometrists, a few years after their training. When the men were examined with tests which they had not seen before, there was no doubt about their color deficiency. The effects of the training were, however, still evident on tests which the men had studied earlier. In a complementary paper, Ross & Mueller (44) identified many of the cues which enabled a color-deficient subject to memorize the American Optical Company test after a few hours of unsupervised study. There was no evidence, however, that the man's color vision had improved. Chapanis (41) has also laid another fiction about color vision defects to rest. He found that errors on pseudo-isochromatic tests were independent of ordinary defects in visual acuity.

The visual literature contains numerous and controversial articles on the heightened chromatic contrast said to be characteristic of color deficiency. If, for example, a deuteranomalous individual looks at a red and yellow field side by side, he frequently calls the yellow "green." In the absence of the red field, he frequently identifies the yellow correctly. The problem here lies in the apparent contradiction that a color-deficient person, who by definition has reduced color sensitivity, should exhibit contrast effects greater than normal. Chapanis (45) has reported some additional data on this point and has suggested an explanation. Chromatic contrast, he says, is some small multiple of an individual's j.n.d. for chromaticity. In the color-normal, compensatory adjustments for contrast are small because the color-normal has small j.n.d.'s; adjustments made by the color-deficient are large because their j.n.d.'s are large. If the task involves color naming, the explanation is partly a semantic one. When the color-deficient person sees a small shift in hue produced by contrast, he assigns a distinctly different name to the sensation, because in associating with color-normal people all his life he has learned that people have markedly different hue names for what look like small differences in hue to him.

The tritanopia of the central fovea.—The tritanopia of the central retina has again been confirmed by Middleton & Holmes (46). Using test patches 1′ and 2′ in size, these authors find a regression of the apparent hues of 20 colors towards a line joining yellow and blue. Even though they used stimuli much smaller than the 30′-size recommended by Willmer, Middleton & Holmes found that the apparent hues of their small colored surfaces did not lie on a single line as nicely as one should expect. This, however, may be the result of the fixation conditions they used. In any event, their results are in essential agreement with earlier work.

Willmer, meanwhile, has continued his researches on the dichromatism of the central fovea and has attempted to distinguish several types of

dichromats. If the fovea of the color-normal person is dichromatic, then—he argues—the fovea of a dichromat ought to be monochromatic. When dichromats are tested with very small colored stimuli, they do, in fact, turn out to be totally color-blind in the fovea (47).

According to a trireceptor theory, dichromatism could occur in several ways: (*a*) the red receptor might be missing, leaving the blue and green receptors—this condition we conventionally call protanopia; (*b*) the green receptor might be missing, leaving blue and red receptors—conventional deuteranopia; or (*c*) the red and green receptors might be both present and active but their fibers might be fused, so that separate red and green impulses never reach higher centers. Since the central fovea is tritanopic, that is, the blue receptor is missing or inactive there, protanopes should have only green (G) receptors, deuteranopes should have only red (R) receptors, and the unconventional dichromats should have red and green (RG) receptors in that area. According to Willmer, RG dichromats could occur in all degrees between R and RG, or between G and RG. Although he does not mention it, dichromats could also occur, theoretically, in all possible variations between R and G. Why he excludes this particular combination is not clear.

RG dichromats should have essentially normal visibility curves in the central fovea; R and G should have aberrant visibility curves; and intermediates between R and RG, or G and RG, should give intermediate visibility curves. In addition, selective adaptation with red and violet lights should distinguish the G or R from the RG dichromat. For the G (or R) the adaptation effects of a red or violet light should be the same on red and violet test lights. For an RG dichromat, however, red adaptation should affect the threshold for red much more than it affects the threshold for violet. Conversely, violet adaptation should affect the threshold for violet much more than it affects the threshold for red. Willmer found dichromats who gave functions to agree with most of these predictions (48) and so concluded that there are the different kinds of dichromats he postulated.

One aspect of Willmer's findings which does not seem to fit is that visibility curves for the R dichromat do not deviate as much from the normal as do visibility curves for the G dichromat. The latter should give some large deviations in the green and blue portions of the spectrum, but they do not. Then, too, the adaptation data which he ascribes to the intermediate deuteranopes (R plus RG) are, in my opinion, not intermediate at all. They are exactly equal to the data for the R dichromats in all respects but one—violet affects violet thresholds more than it should. Finally, whenever one mentions "types," the psychologist naturally thinks of N's and reliability of measurement. Willmer's study was done on 7 normal and 11 dichromatic subjects. Small samples always have been, and probably always will be, the major difficulty with schemes for classifying color-vision defects and this may explain why only one kind of protanope turned up in this experiment. Despite all these objections, Willmer's studies constitute a valuable new approach to an old problem.

Binocular flicker.—The problem of central versus peripheral control of visual functions has had a long and controversial history. In 1904, Sherrington reported that he found no evidence of interaction between the two eyes as evidenced by monocular and binocular CFF's. Using an improved electronic apparatus (49), Ireland has reinvestigated Sherrington's problem by measuring (*a*) monocular CFF's; (*b*) binocular CFF's with the light and dark periods out of phase; and (*c*) binocular CFF's in phase (50). Binocular in-phase stimulation, he found, gave CFF's higher than monocular CFF's and the latter, in turn, were higher than those for binocular out-of-phase stimulation. This seems to provide clear evidence for some central control of flicker. Ireland does not consider the possibility that a sizeable part of the differences he found might be traced to the increased probability of getting fusion or flicker simply because two eyes were used instead of one. Such a statistical explanation accounts for all of the monocular-binocular difference in light threshold [Pirenne (51)], and Smith & Licklider (52) have shown how this problem can be handled for the comparable situation of monaural-binaural comparisons. There is probably some real interaction nonetheless, because the differences Ireland found look too big to be accounted for by a statistical hypothesis.

Visual Perception

Figural aftereffects.—Figural aftereffects continue to stimulate research, most of which does not add up to a clear picture. Köhler & Fishback (53) argue that repeated measurements destroy the Mueller-Lyer illusion by satiation rather than by learning. A number of observations support this view, among them the following: (*a*) the illusion can be destroyed when subjects do not know that they are dealing with an illusion and when they are not told how their results compare with objective facts; (*b*) if experiments are continued beyond the point where the illusion is destroyed, negative illusions may be established, that is, the distance which was once judged greater is now judged less; (*c*) when the illusion is destroyed in one retinal location, it is still present in others; and (*d*) the illusion cannot be destroyed with repeated tachistoscopic exposures. On the basis of these findings, the authors conclude that visual experiments which involve repeated observations of some stimulus pattern should be made with caution because of the danger of contamination by figural aftereffects.

Despite this evidence of the persistent effect of satiation, Hammer (54) found that figural aftereffects (*a*) are at a maximum immediately after the inspection period, (*b*) decrease rapidly with increasing time after inspection, and (*c*) reach zero at about 90 sec. and remain there for any longer times after inspection. The aftereffects increase with increases in the duration of the inspection period to a maximum at about 1 min. Walthall (55) has also generally confirmed some of Köhler's observations on this phenomenon and reported that the strength of satiation varies directly as a function of distance from the satiated to the test areas.

Three studies turned up negative results of one sort or another. Free-

burne & Hamilton (56) found that there are no differences in figural aftereffects with stimulus fields of 100 and 20 foot-candles. Weitz & Compton (57) report equivocal findings in experiments on stereoscopic figural aftereffects and Crutchfield & Edwards (58) found that prolonged fixation of a visual pattern did not affect autokinetic movement differentially when the fixated figure was placed on the same side as the prevailing direction of perceived movement or on the opposite side. One experimenter found the phenomena attendant on long fixation just as interesting as the aftereffects produced by the fixation [Marks (59)]. Four different sorts of phenomena were reported by subjects who observed a large circle near a fixation point: (*a*) movement of the fixation point, or changes in its shape; (*b*) periodic variations in apparent light intensity; (*c*) partial or complete blotting out of the phenomenal field; and (*d*) a tendency for the circle to assume various elliptical or polyangular shapes. Several highly speculative hypotheses are advanced to account for these phenomena.

Perception of the vertical and horizontal.—Recent experiments point to an intimate association between one's perception of the axes of visual space and the position of one's body in space. Witkin (60) built a chair within a small room in such a way that the chair and room could be tilted independently. Subjects were required to adjust either the chair or the room until it appeared upright. Results with a variety of initial stimulus combinations leave no doubt that the perception of the vertical is markedly affected by body orientation, and vice versa. Subjects cannot accurately position the room to vertical if their seats are tilted, nor can they right their seats if the room is tilted. Essentially similar results are reported by Mann, Berthelot-Berry & Dauterive (61).

What happens when the forces acting on one's body are manipulated is not so clear. Witkin (62) had subjects ride inside a small room built on a kind of merry-go-round. While in motion, the subjects were required to adjust a rod to the vertical position. When the room lights were on so that a visual frame of reference was available, the subjects adjusted the rod to roughly its true vertical. When the lights were off, the adjustments were made to an angle midway between the true vertical and the resultant of the centrifugal and gravitational forces. When Noble (63) repeated Witkin's experiments on a centrifuge in the dark, he found the perception of the visual vertical to vary directly as the resultant between the centrifugal and gravitational forces. Noble feels that the discrepancy between his findings and those of Witkin is to be sought in inadequate control of set and adaptation in Witkin's experiment.

Miscellaneous perceptual problems.—How do people perceive the numbers of dots when randomly-arranged groups of them are flashed on a screen for a short time? According to Kaufman, Lord, Reese & Volkmann (64), three different processes might be used: subitizing, estimating, or counting. Subitizing is what occurs when the stimulus number is less than six; estimating occurs when the stimulus number is more than six; counting occurs when the individual has enough time to enumerate the dots serially. Subi-

tizing is a more accurate, more rapid, and more confident process than estimating. It amounts almost to an immediate perception of number. The authors are led to this conclusion by time, error, and confidence (ratings on a 5-point scale) curves, all of which show marked discontinuities at about a stimulus number of six.

According to statements of the horizontal-vertical illusion, vertical lines look longer than horizontal lines of equal length. Shipley, Nann & Penfield (65) confirmed this old finding but they added a new twist. If the horizontal is called 0° and the vertical 90°, then lines tilted at angles of 45°, 60° and 75° look longer than the vertical, with the 60° line having the greatest apparent length. Evidently we shall have to find a more suitable descriptive name for this old illusion.

Meister (66) has attempted to find differences in the way preschool children and adults see figure-ground relationships. There is a considerable amount of Gestalt theorizing in his article but, considering the shaky statistics on which the study rests, it seems much safer to conclude only that the children apparently did not respond in the same way that the adults did—a conclusion which should not excite anyone. The most outstanding difference between the children and adults—a difference which the author does not comment on—is that the adults were apparently much more consistent in their behavior.

Physiological Bases of the Visual Process

Electrical studies of the visual process.—The Brown laboratory is the scene of active studies on the electrical response of the human retina. In one study, Riggs & Johnson (67) reported that the *b*-wave, the principal component of the retinal response, can be identified with rod vision. A fuller treatment of essentially the same kind of data was reported elsewhere (68) and reviewed last year. Johnson (69) then showed that the size of the *b*-wave increases with dark adaptation so that less light is required to produce *b*-waves of certain arbitrary magnitudes. Curves of the latter are roughly analogous to ordinary curves of dark adaptation.

This is fine work and it needs to be done. So far, however, the results are not very satisfying. Curves of "dark adaptation" obtained from the electrical data are only approximately like the dark-adaptation curves one gets in psychophysical studies. Of course, there is no real reason why the two kinds of data should agree closely. The electrical response is exceedingly difficult to elicit, and light flashes of from roughly 100 to 7,500 millilamberts were needed to evoke measurable responses in Johnson's experiment. (Recall that in conventional dark-adaptation experiments light intensities range from about 0.01 to 0.000001 millilamberts, although they may vary by a factor of 100 depending on the area stimulated, size of test light, and so on.) Then too, there is the disturbing fact that electrical "dark-adaptation" curves vary in shape with the criterion measure of response selected. Finally, we know that the color of the test light has a marked effect on ordinary threshold curves of dark adaptation. Johnson did not equate his colored lights

photometrically so we cannot tell whether the electrical data would resemble psychophysical data in this respect. What he did was to equate the intensities of the colored lights in terms of their effectiveness in eliciting electrical responses. When he did this, he found that blue did not fall into the same pattern as red, yellow, and white. At lower intensities, blue was much more effective than one might expect. This sounds like the Purkinje phenomenon, but if it is, why weren't the red and yellow lights displaced with respect to white? The answer to this is anybody's guess. The truth of the matter is that no psychophysical experiments have ever been done with the same stimulating conditions and criteria which Johnson had to use in his work. When the right experiments are done, it may turn out that the correspondence between sensation and electrical response is closer than it appears at the moment.

Motokawa (70) has described a new method for getting at physiological mechanisms of color reception in man. It has been known for some time that electrical stimulation of the eye produces a diffuse sensation of light. Motokawa's technique rests on his discovery that the electrical excitability of the eye increases (less voltage is needed to evoke a phosphene) shortly after photic stimulation and that it then returns to normal during the next few seconds. Moreover, the time of occurrence of maximum supernormal excitability is a function of only one stimulus variable—wavelength of the preceding light. Maximum excitability occurs with shortest latency after stimulation with red light and occurs progressively later after lights of shorter wavelengths. This technique enables Motokawa to (*a*) compare physiological receptor mechanisms in different parts of the retina, (*b*) compare receptor mechanisms in color-normal and color-deficient subjects (71), (*c*) derive a set of three sensitivity curves which are strikingly similar to the König-Dieterici "Grundempfindungskurven," and (*d*) suggest a peripheral, physiological explanation for chromatic and brightness contrast (72). In one of these studies (71), he also finds a yellow receptor in the periphery, but not in the fovea, of the normal eye.

It is difficult to know exactly how to evaluate these findings. They are definitely exciting but are almost too good to be true. Motokawa, unfortunately, is not completely lucid and he does not say precisely how many subjects he tested, how many observations he made at each point, how much variation there is among normal subjects, and what criteria the subjects used when they reported three distinct thresholds of electrical excitability following stimulation with certain wavelengths (this would have to occur in order to get overlapping sensitivity curves, as Motokawa did). Then, too, it is not easy to see how such neat curves for different retinal regions can result from crude electrical stimulation applied to the entire eyeball. Finally, one wonders about the precision of threshold determinations on an event which lasts only 5 sec. Despite these questions, this is a fascinating set of data and, if they can be confirmed independently, we should look for more valuable findings with this new line of inquiry.

Single fiber studies.—Studies of the electrical response of single retinal

fibers are continuing actively and several have appeared from Granit's laboratory during the past year. The most remarkable feature of this new work is that it shows that the same isolated retinal element may respond differently to lights of different wavelengths. Donner (73) found that when well-isolated elements in the retina of the cat were stimulated with different wavelengths, the on-elements gave the same kind of response to all wavelengths. Many of the on-off and pure off-elements also behaved in the same way. But in some on-off and pure off-elements the frequency of responses immediately following stimulation with red light was much greater than the frequency following stimulation with green and blue lights. If the frequency of impulses is counted a little later (0.2 sec.) after the cessation of the light, the green stimulus gives the strongest effect. The "blue" effect occurs still later. Donner also stimulated elements with wavelengths from 12 different parts of the spectrum and counted the frequency of impulses occurring within a 0.1-second period 0.03 sec. after the cessation of stimulation, 0.17 sec. after, and 0.24 sec. after. These curves show three maxima: in the blue, the green, and the red parts of the spectrum.

In a similar vein, Gernandt (74) studied the adaptation effects of prolonged stimulation with white light on the thresholds of isolated retinal elements tested with red, green, and blue lights. The results show that all fibers do not respond in the same way. Some show a much greater elevation in red thresholds than in green and blue thresholds. Others show the greatest elevation in green thresholds and still others show the maximum elevation in blue thresholds. Granit (75) has explored the interaction which occurs when single fibers which respond differently to different wavelengths are stimulated simultaneously with two different wavelengths. Finally, Donner & Granit (76) have studied further the effect of illumination on the sensitivity of isolated retinal elements to polarization. The implications of all these findings is that visual purple—if it is visual purple which is responsible for all these differential wavelength effects—must exist in different states in the eye at the same time.

Photochemistry of the retina.—Recent advances in this area lead us to expect that we shall soon have the complete story on the rhodopsin-retinene-vitamin A cycle. Vitamin A_1, it turns out, is an alcohol, and retinene$_1$ is its aldehyde [Wald (77)]. Retinene$_1$, in fact, can be produced synthetically from vitamin A_1 by oxidation. So the cycle goes something like this: rhodopsin is bleached by light to retinene$_1$ which in turn is converted to vitamin A_1 by a reaction for which reduced cozymase serves as coenzyme.

As is well-known, the rods of fresh-water fishes, lampreys, and some amphibia contain porphyropsin instead of rhodopsin. Porphyropsin has its peak of maximum absorption somewhat higher in the spectrum than does rhodopsin (at about 540 mμ instead of 500mμ) and it is converted to retinene$_2$ and vitamin A_2 in the same manner as in the rhodopsin cycle. (Retinene$_2$ and vitamin A_2 both have absorption spectra with maxima higher in the spectrum than their analogous rhodopsin products.) These new researches by Wald show that the apoenzyme from fishes reduces retinene$_1$ to vitamin A_1

just as effectively as it converts retinene$_2$ to vitamin A$_2$. Conversely, the apoenzyme from frog retinae (whose rods contain rhodopsin) works just as well on the porphyropsin system. Incidentally, the central component of the retinene reductase is nicotinamide, a member of the vitamin B complex.

The absorption of light by rhodopsin results in bleaching and in the initiation of a neural response. At first glance, it might appear that the bleaching is the source of the visual excitation. Actually, the formation of retinene$_1$ is much too slow a process to accord with the facts of rod vision, and researches by Wald, Durell & St. George (78) show that the true state of affairs is far more complex. If dried gelatine films are prepared from rhodopsin, the initial response to bleaching with light is a small shift of about 6 mμ in maximum absorption toward the blue and a small rise in the maximum (about 10 per cent). This new product is called lumi-rhodopsin. Within the next 15 min., in light or in darkness, lumi-rhodopsin shifts in maximum absorption toward the blue by an additional 5 to 10 mμ and declines slightly in height. This is meta-rhodopsin and it remains stable for days, in light or in darkness so long as it is dry. When the film is wetted, meta-rhodopsin immediately goes on in the dark to retinene$_1$ and rhodopsin in about equal parts. Exactly the same sequence can be observed in super-cooled solutions of rhodopsin. The analogy with the latent image formed on photographic film by light, and the subsequent "developing" of the dry film in a solution is striking. The important point is that the initial light reaction, the conversion of rhodopsin to lumi-rhodopsin, is a small effect, and it is this which probably initiates the visual excitation.

Applied Visual Research

The last three annual reviews of vision (1, 2, 3) have emphasized the amount of applied visual research being sponsored by Federal agencies. Psychologists have always applied their information about vision to reading, the selection of workers for industry, and related problems, but the years since the war have seen the development of a new kind of applied research. This is best described as the application of visual data to engineering design. These studies have been concerned with the design of dials, the presentation of complex symbolic information in visual displays, visibility and contrast on radar scopes, and so on. Most of these researches appeared originally in mimeographed form and in a limited number of copies. It is gratifying to see that many have now been published in journals. Sponsored research, or useful research, is not synonymous with applied research. Many studies done under research contracts are reviewed elsewhere in this paper.

Studies on dials.—Problems of dial design have received considerable attention and, during the last year, Grether (79) and Grether & Williams (80) have reported data in this area. The former paper is concerned with reading errors on multirevolution dials and with dial designs which largely eliminate these reading errors. The latter paper shows that the optimum spacing between dial markers is about a half inch. Especially interesting in this con-

nection is the finding that for at least one kind of aircraft instrument, pictorial displays are superior to symbolic displays—those which present information in terms of dial readings, pointers, and numbers [Williams & Roscoe (81)].

The specificity of much of this applied research makes general principles hard to find. Garner & Gebhard (82), however, have been able to consolidate data from several studies and prepare a nomograph which predicts the percentage of correct readings on dials as a function of the size of the marked interval and the size of the unit to which a reading is made. This is applied science at its best and it shows that broad and useful generalizations should be possible eventually from much of this work.

Related to this general problem is a study by Fitts, Jones & Milton (83), who photographed and analyzed eye movements of pilots in aircraft to show the relative importance of various instruments and dials for standard types of maneuvers. Finally, Brown & Poppen (84) have done some nice work on the vexing problems of illumination of dials and the legibility of different kinds of symbols for dials and instruments.

Scales for radar scopes.—Ford (85, 86) has been concerned with scales as they are applied to certain kinds of radar scopes and in the first study he found large errors in the estimation of target positions due to the perspective illusion created by a triangular display. The illusion can be overcome by redesigning the shape of the display or by putting in multiple scale markers. The latter solution introduces its own difficulties, however, since subjects now become prone to errors of confusion in the identification of particular scale markers. Bartlett, Reed & Duvoisin (87) found that perception cannot be divorced from its semantic aspects. The relative accuracy of visual interpolation between markers is influenced in part by the particular numbers assigned to the markers. Scales numbered 20, 40 can be used with relatively more accuracy than scales numbered 10, 20, and the latter in turn are better than scales numbered 5, 10.

Visibility and contrast on radar scopes.—Problems of visibility and contrast on cathode-ray-tube screens are still being studied by the Hopkins Laboratory and experiments by Harriman (88), Harriman & Williams (89), and Williams (90) were published in this area during the last year.

Reviews of human engineering data.—Enough information on applied problems has now been accumulated to make reviews of it worthwhile. A noteworthy publication in this respect is *The Handbook of Human Engineering Data for Design Engineers* (91), prepared by psychologists at Tufts College under contract with the Navy Department. This volume represents an attempt to collect useful information about the psychological characteristics of people and present it in an understandable form for design engineers. As one might perhaps expect, the largest section of this volume is devoted to vision. This part of the *Handbook* deals with some basic characteristics of vision (light sensitivity, visual acuity, depth perception, color vision, flicker, and the like); factors affecting the efficiency of vision (illumination and atmospheric optics); and applications of visual data (legibility of indi-

cators and printed material). The *Handbook* falls short of its goal but in large part this is due to the nature of the data available. Psychologists have amassed an enormous number of experiments on vision but a large proportion of them are concerned with rather useless problems. Then, too, psychologists are long on theory and short on data. These faults are glaringly apparent when one tries to put together something like a handbook. There are too few solid facts, accumulated on at least a fair sample of subjects, to make much of a showing. Perhaps this applied nture will stimulate more interest in the collection of dependable data w h, in turn, should put visual theory on a much firmer ground. The *Handbook*, incidentally, was printed in only 500 copies. Later editions, we hope, will be more generally available.

Visual requirements in industry.—The relationship between visual capacity and accident proneness in a large optical company is the subject of an article by Tiffin, Parker & Habersat (92). These investigators compared the visual performance of 42 "accident-prone" employees with that of 42 "accident-free" employees who had been paired and matched with the former group in age, education, and experience. The sixteen measures of visual performance show mean differences in the right direction but, taken by itself, this is not an impressive study. Only one difference is statistically significant at the 5 per cent level and two at the 1 per cent level. The two which are significant at the 1 per cent level are both near acuity scores (one for the right eye, the other for the worse eye) which should be highly correlated anyway. The trouble with all this is that if you take 16 differences at random, the probability of finding a difference significant at the "1 per cent level," as computed with ordinary probability tables, is better than 1 in 10. (It is difficult to calculate exact probabilities because of the intercorrelations among many of these visual measures.) For this reason, it is important to view these findings with the appropriate amount of skepticism.

Whatever doubts we might have about individual studies in this area are dispelled when we consider the results of a number of such studies. McCormick (93) dug into the files at Purdue and came up with records on 5,500 employees in 92 different jobs—each of the jobs represented by at least 20 workers. When the data are analyzed by jobs, and not by individuals within jobs, it seems clear that for most jobs high-criterion workers have better visual capacities than poor-criterion workers.

Kephart & Besnard (94) have constructed a test which discriminates reliably among individuals in their capacity to differentiate two kinds of marbles which roll down an inclined trough. The authors do not say what good the test is, for their philosophy seems to be to build a test first and find out later if it will predict anything.

Visual fatigue.—Tinker (95) had 42 subjects read for 55 min. under 2 and 100 ft. candles of illumination and found that the blink rate increased exactly the same amount under the two intensities. This leads Tinker to conclude that the blink rate does not reflect differences in ease of seeing. Contrary findings are reported by Brožek, Simonson & Keys (96) who showed that the blink rate changed as expected in two-hour experiments under 5 to 300

foot candles of illumination. The studies by Brožek *et al.* actually measured a large number of changes in visual capacity following 2 and 4 hr. of strenuous visual work. Although several measures (CFF, length of the fixation pause, and extent of movement during fixation in voluntary eye-movements) show reliable changes after fatigue, many visual measures show no change at all. The most consistent changes to occur with fatigue were in the over-all performance level in the work task itself and in the number of complaints the subjects registered.

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HEARING

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The Anatomy of the Ear

Throughout the history of the study of hearing, our true understanding of it has rested upon the foundation of anatomical knowledge. There have been four notable contributions to this knowledge during the past year.

(*a*) De Vries (1) by a new method dealt with the old problem of the form and normal position of the tectorial membrane in the cochlea. He plunged fresh temporal bones into liquid air, which quickly froze the tissues to a state of brittleness that enabled him to break them up and disclose the innermost structures. The tectorial membrane was found to be large and expanded, with much the form revealed earlier in Hardesty's (2) studies. This observation confirms the usual opinion that the standard histological techniques produce a gross shrinkage and distortion of this structure. De Vries also observed that this membrane lay in direct contact with the organ of Corti, from which it was easily dislodged with a needle. He inferred that it was attached to the organ of Corti only by the cilia of the hair cells. Further details of structure of the tectorial membrane were revealed on examination with the phase microscope.

(*b*) Gisselsson (3) investigated the relations of the endolymph and perilymph to one another and to cerebrospinal fluid and the blood stream by the injection of a dye, sodium fluorescein. After the dye was injected into the blood stream it soon appeared in the cerebrospinal fluid, but ordinarily did not penetrate to the endolymph or perilymph until several hours had passed. Yet if the labyrinth was opened, as, for example, by puncturing the round window, the dye promptly appeared in the perilymph. Gisselsson explained his results as due to the presence of Waltner's (4) membrane which normally forms a barrier at the mouth of the cochlear aqueduct (perilymphatic duct) and prevents the passage of cerebrospinal fluid. On the opening of the labyrinth, he supposed, the membrane ruptures and permits this passage.

This explanation encounters two difficulties. In the first place, I do not believe that Waltner's membrane exists. In the second place, such a membrane is not necessary to account for Gisselsson's observations. Any conduit will carry fluid faster if an end is opened and a direct flow is substituted for mere diffusion. Further, it should be mentioned that this type of experiment is fraught with hazards, for after a dye is introduced into the blood stream it is not possible to gain access to either perilymph or endolymph without exposing blood capillaries that carry the dye. It is therefore well-nigh impossible to determine whether the dye was in the cochlear fluids or not before the opening to them was made (5).

Gisselsson concluded from his observations that the endolymph is

secreted in the membranous labyrinth, as had been indicated by Guild's (6) experiment several years ago. Bast (7) has added indirect support to this view, in pointing out that the cells lining the endolymphatic sac have the appearance of an absorption epithelium.

(*c*) Bast & Anson (8), in a book, *The Temporal Bone and the Ear*, have presented a thoroughgoing treatment of the anatomy of the ear. The approach is embryological, with extensive use of human fetal material. The emphasis is upon the normal anatomy, but two chapters are devoted to pathological processes.

(*d*) Tunturi (9) has continued his explorations of the auditory area of the cerebral cortex in dogs. In his recent study, he sought to delimit this area by the local use of strychnine, which facilitates the production of electrocortical potentials in response to sounds. By this method he found the boundaries of the acoustic cortex to be the suprasylvian and ectosylvian gyri.

The Mechanics of the Middle Ear Apparatus

There has been considerable interest in the middle ear, its mechanical properties, and its contributions to the sensitivity function. It seems that at long last, by the accumulated power of experimental evidence, we are about to unburden ourselves of Helmholtz' domination of this field, just as is beginning to happen likewise in the field of cochlear mechanics. The results of a long series of experiments prove that the middle ear system is an efficient, closely coupled, and highly faithful transmitter of acoustic vibrations and not the crude mechanism that was formerly represented. When driven by sounds within normal physiological limits, it conveys them to the inner ear with no measurable distortion, and even when the sounds are excessive its distortion is small in relation to what then occurs in the organ of Corti.

Details of the action of this mechanical system are brought out in studies by Wever, Lawrence & Smith (10) and by Wever & Lawrence (11, 12, 13). As a mechanical transformer, this system facilitates the conduction of sounds to the inner ear by an amount of 28 db. on the average (as measured in the cat). Yet it performs this service with remarkably little disturbance of the response pattern. It does so because its impedance varies only moderately with frequency, as shown by measurements of both the magnitude and phase changes for which it is responsible.

Of late, clinicians, in their efforts to account for pathological conditions, have shown much interest in the physical principles governing operation of the middle ear. Campbell (14) discussed the applications of the "impedance formula," which represents the ear's properties as single lumps of mass, stiffness, and friction. This is an oversimplification, and yet the conditions are not so complex as to defy treatment, as some have supposed. The evidence indicates that the middle ear can be satisfactorily represented by 3 masses, 4 stiffnesses, and 2 or 3 frictional elements. Onchi (15) attempted a mathematical treatment of the middle ear, but found his equations too compli-

cated to handle. He came out with conclusions that follow his original assumptions.

Sullivan & Hodges (16) dealt with the problem of the production of basilar membrane movements through the action of sounds, especially sounds presented by bone conduction. They described one of the two principal means of bone-conduction stimulation outlined earlier by Bárány (17).

Jones & Edmonds (18) studied the effects on human ears of positive and negative pressures introduced into the external auditory meatus through a special plug attached to a telephone receiver. Sensitivity to sounds by air and bone conduction was tested as a function of the pressure changes. The results were rather irregular. A complication in this experiment arises from the fact that the output of a telephone receiver is altered by a change in the static pressure on its diaphragm (19).

Kobrak (20) studied the characteristics of the round window membrane by reflecting light from a mirror cemented to its surface. With the middle ear intact he measured the deflections of this membrane while stimulating with static pressures and sounds at the external auditory meatus. He reported greater deflections for negative than for positive pressures, and concluded in favor of the common view that the ossicular system operates asymmetrically. However, it must be noted that his pressures were enormous, exceeding 1,000 dynes per sq. cm. Any mechanical system, of course, can be made to distort if overdriven.

The application of our knowledge of the middle ear mechanics to ear disorders, and especially the problem of otosclerosis, will be discussed in a later section.

The Cochlear Potentials

Several studies have dealt with the nature of the cochlear potentials, their relations to auditory nerve excitation, and their practical applications.

Hamberger and his co-workers (21) reported some exploratory studies of cochlear potentials in the guinea pig. They recorded largely from the bony surface at the apical end of the cochlea and obtained a nonlinear function with sound intensity. Their curves appear to represent the upper portion only of this function, which is indeed nonlinear on account of overloading.

Davis, Gernandt, Riesco-MacClure & Covell (22) also reported results of a preliminary nature on the guinea pig, which mostly confirm older observations. A curious observation was the generation of a subharmonic as a result of stimulation with strong tones of frequencies above 2,500 cycles; this is supposed to have arisen from a special mode of vibration of the ossicles.

Davis, Gernandt & Riesco-MacClure (23), in a further investigation, sought to isolate the cochlear and nerve potentials obtained from the region of the cochlea. They used surgical injuries and quinine to eliminate the nerve potentials, and then measured the 'thresholds' of the cochlear potentials. To isolate the nerve potentials they introduced a sinusoidal current in phase opposition to the cochlear component.

Further observations of cochlear potentials were carried out on the pigeon

by Bleeker & de Vries (24). In most respects their results agree with previous ones. As regards the effects of movements of the bill, however, their observations are contrary to those of Bray & Thurlow (25). De Vries & Bleeker (26) found the responses to continue at a low level after the cochlea was destroyed, and attribute these effects to the sensory organs of the cristae and perhaps of the saccule and utricle as well.

Gisselsson (27, 28) considered the problem of excitation of the cochlear nerve fibers, and especially the question whether a humoral substance acts as an intermediary in this process. He found, as Martini (29) had previously, that perilymph and endolymph are capable of breaking down acetylcholine. However, he was unable to verify the claim that sound stimulation enhances this reaction. He found further that substances like physostigmine that are capable of destroying acetylcholine esterase, prolong the latency of the cochlear potentials, presumably because they permit a high accumulation of acetylcholine. Gisselsson concluded from these results that acetylcholine participates in the change of sound energy to electrical energy. Consistent with this position is the observation of Bornschein & Krejci (30) that perfusion of circulatory stimulants like acetylcholine and histamine caused a mild reduction of cochlear potentials.

Lempert, Meltzer, Wever & Lawrence (31) reported further investigations of the cochlear potentials in man. They conclude that the technical difficulties prevent the practical use of such potentials in clinical diagnosis.

Other studies have been concerned with the effects of oxygen deprivation upon the cochlear potentials. Wever, Lawrence, Hemphill & Straut (32) found that severe deprivation, almost to the point of failure of the heart, causes a rapid loss of cochlear potentials. If the loss does not proceed too far, the responses are fully recoverable, but the larger losses, of 20 db. or more, are recovered only in part. Repeated anoxemias have accumulative effects. These changes are found for all tones, without any appreciable variation with frequency. Similar observations were made by Bornschein & Krejci (33, 34) and by Riesco-MacClure, Davis, Gernandt & Covell (35).

Audiometry

A number of studies have been concerned with the problems of auditory testing. A general review of this subject was given by Lewy, Shapiro & Leshin (36). Comments on techniques and sources of error were made by Reger (37) and by Myers & Harris (38). The latter investigators found the short-term fluctuations of the threshold to be of the order of 1 db., and recommended the use of smaller steps of intensity than those provided by the usual audiometers. Contrary to usual impressions, they found tests by bone conduction to be as reliable as those by air conduction, and tests with high tones as reliable as those with low tones, when the proper precautions are taken. Carhart & Hayes (39) likewise found bone-conduction results to be fully acceptable, and perhaps actually better than air-conduction results.

The clinical use of speech tests is attaining a new popularity following refinements in technical equipment. Harris (40) pointed out the advantages of

instrumental monitoring of the voice and the use of recordings. Breakey & Davis (41) compared binaural with uniaural listening with recorded word lists, and found binaural listening to give greater sensitivity by about 3 db.

Miller & Licklider (42) measured the intelligibility of speech when interrupted in various ways: by periodic intervals of silence, or intervals of noise, or masking with noise. Bruīne-Altes & Huizing (43) mentioned six methods of testing loudness recruitment, and made special reference to the uniaural masking method. The problem of auditory fatigue and the prediction of damage to the ears from prolonged exposure to loud sounds has been discussed by Theilgaard (44). Bordley & Hardy (45) reported results on the testing of hearing by the observation of psychogalvanic responses after conditioning with electric shocks. It is hoped that this method will be of use in the testing of young children, but so far it is plagued with many difficulties.

Loudness and Masking

Progress continues in the study of the difficult problems of loudness and masking. Doughty (46) discussed the effect of psychophysical method on the determination of pitch and loudness functions. According to his results, the method of constant stimuli and the method of average error give about the same difference limens, but the method of average error is the more reliable for indications of a constant error.

Howes (47) studied the loudness of complex tones, consisting of as many as 11 sinusoidal components, and found [cf. Fletcher & Munson (48)] that the loudness is the sum of the loudnesses of the components provided that no masking occurs. Garner (49) investigated the loudness of tones as a function of their duration and also of the time interval between the two tones being compared.

In the earlier studies of Fletcher & Munson (50, 51), the cochlear response patterns on which loudness depends have been inferred on the basis of masking data. A difficulty in this method, which was well recognized by these investigators, arises from the presence of beats between primary and masking tones in the region of the primary frequency and its multiples. Munson & Gardner (52) have employed a new method that avoids this difficulty. This is a residual masking method: the masking tone is presented alone, and, at a specified time following its removal, a 'probe tone' is introduced. This probe tone is impaired in loudness on account of the residual effects of the first tone, and to be perceived must be raised in intensity above its usual threshold. The use of probe tones of various frequencies reveals the residual masking pattern. It is of interest that the patterns developed in this manner are much narrower than those indicated by physiological data. The authors are of the opinion that neural factors play a part in increasing the selectivity of the action. Similar results were obtained by Lüscher & Zwislocki (53) by this same method.

Hawkins & Stevens (54) repeated the work of Fletcher and Munson on the masking effects of thermal noise, with results in close agreement with those obtained earlier. They also determined the threshold of intelligibility

for speech in the presence of noise of various levels. Shafer & Gales (55) compared the ability of observers to detect complex tones (made up of 2 to 8 components) in the presence of a masking noise to their ability to detect single tones under the same conditions. The complex tones are detected more readily, especially when their structure has a certain form quality.

Zwislocki (56) investigated the loudness function for bone-conducted sounds. It has been reported that these sounds rise more rapidly in loudness than air-conducted sounds as the intensity is raised above the threshold values. Zwislocki found this difference only when one mode of stimulation involves one ear and the other mode involves both ears. The loudness functions are the same for air and bone-conduction when the stimulation is either uniaural or binaural for both.

Binaural Phenomena

Considerations of sense perception in general and of hearing in particular have always emphasized the peripheral mechanisms almost to the exclusion of neural factors, but the insistent appearance of binaural phenomena forces us out of this narrow position. Numerous studies prove that effects mediated by the two ears are compounded in various ways. Smith & Licklider (57) recently argued that this compounding is actually a supplementation rather than a summation, but the evidence is somewhat limited for a final decision on the point.

Wallach, Newman & Rosenzweig (58) investigated the maximum time interval between two binaural stimuli that will still permit a fusion to one apparent sound. This interval is greater for complex sounds than for simple tones.

Hirsh & Webster (59) have continued their work dealing with the effects that interaural phase differences have on the ability to hear a tone in the presence of masking sounds. It has been shown that if the tone is in phase in the two ears it is more easily detected if the masking sound is out of phase in the two ears, and vice versa; in other words, a difference in the apparent location of the two sounds is an aid to their discrimination. This principle operates clearly for tones in the presence of irregular masking sounds like noises and irregular pulses. It fails when the masking sounds are steady tones or regular pulses.

Hirsh (60) continued also the study of interaural phase and the intelligibility of speech. When real sources are used for speech and noise the results are less certain than when the conditions are established with earphones, but in general it appears that a spatial separation of the two signals aids in their discrimination.

Licklider & Webster (61) studied the ability to appreciate binaural phase differences under rather complicated conditions. They presented two sine waves to each ear and shifted the interaural phase relation of one of these waves. They found that the phase shifts could be detected under two conditions: (*a*) when the component that was shifted in phase had a frequency below 1,400 cycles, the shift could be detected regardless of the fre-

quency of the other component; (*b*) when the component that was shifted in phase had a higher frequency, however, the shift could be detected only if the other component had a frequency close to the frequency of the first component. Clearly, in the first condition the invariable component is superfluous, and the subject is simply perceiving binaural phase differences. In the second condition, when the shifted tone is higher in frequency, this direct perception is difficult or impossible, and instead the subject bases his judgment upon the complex wave forms, which differ at the two ears at any instant. These results are explained in line with the volley theory (discussed below). The incidence of nerve impulses is different in the two ears when the phase is made to differ.

Auditory Disorders

Numerous studies have dealt with various aspects of stimulation deafness. Alexander & Githler (62) investigated by means of the cochlear potentials the effects of jet engine noise on the hearing of guinea pigs. Animals exposed for 15 min. to these sounds showed a serious impairment of responses throughout the frequency range. In most of the animals a partial recovery occurred over a period of 3 to 6 weeks, but in a few there were indications of a delayed degeneration. After this type of injury the ears showed an increased susceptibility to further damage by sounds that ordinarily are innocuous.

Discussions of the problem of a temporary loss of acuity in human ears from exposure to noise were made by Wheeler (63) and by de Almeida (64). Davis, Parrack & Eldredge (65) pointed out that sounds of frequencies above the limit of hearing are not particularly dangerous unless of extraordinary intensity. McReynolds, Guilford & Chase (66) reported observations on blast injuries caused by the Texas City disaster, and concluded that drum perforations are caused by the positive pressure wave of a blast.

Hamberger, Hydén & Nilsson (67) exposed guinea pigs for 3 hr. to a tone of moderately high intensity, and found no evidence of an impairment of cochlear potentials. Yet these ears revealed extensive chemical changes in the nerve cells of the cochlear ganglion. These changes, therefore, reflect a physiological process in response to increased functional demands. In part, these changes consist of an exhaustion of nucleic acids.

Several papers have been concerned with various aspects of the problem of deafness. Morrissett (68) criticized present methods of treating nerve deafness and advocated a rehabilitation procedure. Brown (69) added new evidence to that already available on the hereditary character of certain disorders of the ear. Ireland (70) reported on deafness associated with eighth nerve tumors. Sataloff (71) described a curious case of objective tinnitus that was ascribed to a circulatory defect.

Perlman (72) reported changes of bone formation in the ears of rabbits as a result of a diet depleted in vitamin A. Also, he sometimes found degenerative changes in the eighth nerve, and reduced reflexes of the middle ear muscles in response to sounds. This area of study remains confused, as other

investigators using even more severe avitaminosis have failed to obtain auditory losses. Thus, Lawrence (73), using general bodily responses, cochlear potentials, and auditory nerve responses, failed to observe any variations from the normal.

A matter of great interest in clinical otology is the disease of otosclerosis and its surgical treatment. General reviews have been offered by Fowler (74) and by Juers, Derlacki & Shambaugh (75, 76). Techniques have been discussed by Holmgren (77), Sourdille (78), and Lempert (79). Holmgren continues to argue that the gain in hearing produced by fenestration is the result of a loss of perilymph and the relief of an excessive fluid pressure in the labyrinth. Yet there has never been any evidence that this pressure is excessive, and experiments by Lempert, Wever, Lawrence & Meltzer (80) show that an artificially increased pressure does not reduce the conduction of sounds to the cochlea or the generation of cochlear potentials. Sourdille persists in his claims for the multi-stage operation, despite the abundant evidence favoring Lempert's one-stage procedure. However, it appears that his position is largely based on hypothetical considerations, and that his practice of the surgery is exceedingly limited. On the other hand, Lempert and other American otologists trained by him have performed several thousand fenestration operations with a high percentage of success. Some individual experiences are recounted by Hall (81), Lumsden (82), and Shambaugh (83).

Otosclerosis and its treatment present many problems. Nylén (84) discussed the incidence and locus of the bone changes that are the immediate cause of this disease, somewhat as Guild (85) had done some years ago. Lempert & Wolff (86) presented the results of a histological examination of the ossicles removed in the course of fenestration operations, and from these results concluded that the pathological changes in otosclerosis are of circulatory origin. Anson & Bast (87) reported on the embryological development of the cochlear capsule and pointed out some peculiarities in the region of bone over the lateral semicircular canal where the fenestration for otosclerosis is commonly made. We have in these studies a gradual building of knowledge about the pathology of this disease which should lead eventually to a better understanding of its nature and causes.

Otosclerosis is of special interest in relation to the problem of sound conduction in the ear. It is necessary to explain both the losses of sensitivity caused by the disease and the recovery produced in its surgical treatment. These problems naturally give accent to the problems of normal conduction. Recent discussions have been made by Lawrence (88) and by Wever (89) on the basis of studies of middle ear functions already referred to. It appears that the losses of sensitivity observed in otosclerosis come from three principal physical changes that result from a fixation of the footplate of the stapes in the oval window: (*a*) an increased stiffness of the ossicular chain, (*b*) a reflected impedance of the drum membrane, and (*c*) an immobilization of the cochlear fluids. Sometimes, also, there is added a phase cancellation effect from the simultaneous entrance of sounds through oval and round windows.

An understanding of these physical conditions may aid in an improvement of the surgical procedures designed to eliminate their deleterious effects. Other diseases of the ear that are also of the conductive type are beginning to be better understood as a result of modern researches.

Auditory Theory

There have been two main theoretical developments during the past year. One is a further elaboration of the hydrodynamics of the cochlea, represented in the theories of Zwislocki and of Peterson and Bogert.

Zwislocki (90) has continued his treatment of cochlear action in a monograph that presents his position in complete form. This theory belongs to the class of nonresonance place or wave theories. According to it, the differential action of the cochlea occurs mainly because of variations in elasticity, which is assumed to diminish from the basal end toward the apex. The response to a given tone spreads over the cochlea with a broad maximum whose general locus varies with the frequency in the usual manner, moving in the basal direction as frequency rises. For this theory it is necessary to assume a rather low velocity of wave propagation in the cochlea and a progressive falling off in this velocity as the wave approaches the apex.

In Zwislocki's opinion the degree of differential action in the cochlea is not sufficient to account for pitch discrimination. He rejects Békésy's attempt to obtain a secondary refinement of the mechanical action by means of local eddies in the cochlear fluid. Rather, he finds it necessary to assume a more delicate analytic process farther along, in the auditory nervous system.

The theory brought out by Peterson & Bogert (91) is likewise a hydrodynamic theory, and resembles Zwislocki's in some respects. According to this theory a sound wave moves through the two cochlear scalas with high and uniform velocity, the velocity to be expected of a sound in fluid. However, the cochlear duct transmits along its extent another wave motion at a slower and constantly diminishing velocity. This wave motion arises because there is a pressure difference between the two scalas. This pressure difference has a certain pattern of variation along the cochlea for every frequency and therefore the basilar membrane and other parts of the cochlear duct undergo vertical movements of a characteristic sort.

The patterns of pressure differences arise on account of local variations in the mechanical characteristics of the cochlear duct. These characteristics include the mass of the fluid contained in the duct, the stiffness of the basilar membrane, and the friction of the fluid. However, for the sake of simplification the effects of friction were neglected in this treatment.

The mathematical details of this theory were worked out on the basis of Békésy's data on the stiffness of the basilar membrane and measurements of the dimensions of the basilar membrane and the cochlear duct. The computations indicate a mode of response of the basilar membrane much like that brought out in other wave theories: a broad response that attains a maximum at some place in the cochlea according to the frequency. From the

basal end of the cochlea the amplitude of response rises slowly to a maximum, and then falls rather quickly to zero. A peculiarity appears in the form of localization for the low tones. The extreme low tones have their maxima a little below the apical end. As frequency rises the maxima move first toward the apex and then, as the frequency rises further, move down the cochlea in the orthodox fashion.

Two of the basic assumptions of this theory require special attention. One is the assumption that the dissipation of energy through friction can be neglected without seriously affecting the pattern of cochlear action. The other assumption is that the basilar membrane can be treated as an assembly of independent pistons, each operating by reason of its own qualities without being affected by its neighbors. These are very weighty assumptions, and there is more than a doubt of their validity. Peterson & Bogert are quite aware of their difficulties here and suggest that the next steps in the development of the theory are to work out the effects of dissipation and to treat the basilar membrane as a membrane. These important steps will immeasurably complicate a mathematical treatment that is not exactly simple at the present time.

This theoretical development has the virtue of showing that if the fundamental assumptions are true it is possible for the cochlea to produce differential patterns of action in response to various tones. We must not, of course, take the details too seriously. The assumptions are somewhat arbitrary and may have to be changed, and the observational data are very meager. The data presented by Békésy, which form the basis of this and most other such developments nowadays, were obtained by an exceedingly difficult technique on dead tissues or on mechanical models.

The direct mechanical approach to the mysteries of the cochlea, of course, needs to be continued with vigor, but it is my opinion, for whatever it may be worth, that we shall come closer to an understanding of our problem by functional methods: by methods that involve the cochlea in use. One such method is the study of masking by Munson & Gardner (52), already described. Another is the employment of cochlear potentials to determine the response patterns (92).

The second theoretical development is represented in a book entitled *Theory of Hearing* by Wever (93). This book is divided into three parts. Its first part is historical and is concerned with the classical theories and their emergence from early anatomical and physiological knowledge. Its second part covers the modern developments and brings the principal theories to face with recent discoveries. Its third part is a development and evaluation of the volley theory. The thesis that is developed is that the conventional place and frequency theories both have virtues, yet both are defective in certain respects. A full and satisfactory account of auditory phenomena requires a combining and compromising of their basic principles.

The volley theory admits of selective action in the cochlea, but only of a general sort. Also, the selectivity is a function of frequency, increasing for the high tones. The theory depends heavily upon characteristics of the

neural mechanism to supplement the peripheral processes in the production of nearly every type of auditory phenomenon. The form of the sensitivity function, in particular, is considered as reflecting strongly the characteristics of the auditory nervous system.

Certain developments of the past year add support to this position. Already mentioned in this regard are the observations of Munson & Gardner on loudness patterns, which led them to assume a supplementation of the mechanical discrimination of the ear by a neural discrimination. The evidence on binaural phenomena, especially that of Licklider & Webster on interaural phase effects, can only be explained on the basis of a neural representation of temporal characteristics of the stimulus, and a representation of a fidelity that is possible only by the operation of the frequency principle.

Further support comes from study of the auditory nerve and a demonstration of summation in the excitatory process for high tones. This summation was predicted on the basis of Katz' evidence on the excitation of nerves by periodic electric currents, and indeed was found essential to the volley theory for an explanation of the particular nature of the ear's sensitivity to high tones. Now Davis (94) has reported direct evidence of such summation in the auditory nerve responses of the guinea pig for tones above 2,000 cycles.

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SOMESTHESIS AND THE CHEMICAL SENSES

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The cutaneous senses, proprioception, taste, and smell have been somewhat more actively investigated during the past year than in the period covered by the 1950 review. About half of the 87 references in the present review are concerned with somesthesis. Of the latter, many are concerned with the pain or some aspect of clinical neurology. Somewhat more references will be found under taste than olfaction. This may reflect the writer's personal interest. In none of these fields has there been a strikingly new development. The research effort, rather, is a continuation of the different lines of investigation initiated in previous years.

Somesthesis

The present reviewer has found it convenient to divide the work in this field into four categories. The first two are concerned with afferent processes and central mechanisms respectively, excluding pain. Most of this work is highly physiological in orientation. The third category concerns research on pain carried out largely by workers in, or closely allied to, the clinical area. Two major interests may be noted here. One stems from the development and use of the Hardy-Wolff-Goodell radiant heat algesiometer; the other stems from the work of Weddell and his colleagues on the neuroanatomy of the skin. The fourth category is concerned with studies of sensory "extinction" in cases with damage or injury to the parietal lobe. The rather extensive literature on postural and vestibular factors concerned with bodily orientation has been left for a later reviewer.

Afferent processes.—The electrophysiological method of recording afferent impulses from the appropriate nerves has been applied to the study of cutaneous, visceral, and proprioceptive endings (1 to 5). The Pacinian corpuscle in the cat's abdominal cavity makes an intriguing object of study because the individual corpuscles can be seen with the naked eye in the transparent mesentery. At the same time, discharges in their individual axons can be recorded. The minimum mechanical displacement required to excite the single corpuscle was found to be 0.5 micra in 100 micro seconds. From an analysis of the latency of discharge, of changes in excitability due to subthreshold conditioning stimuli, etc., the authors conclude that no intermediary process seems to be concerned in stimulating the nerve. The ending behaves in a manner consistent with the known properties of axons. According to Gray & Malcolm (2, 3), the corpuscle serves merely to apply mechanical stimulation to the axon.

The old question concerning the existence of muscle spindles in the extra-

ocular muscles seems to have been definitely settled. Cooper & Daniel (6) present good evidence for the existence of eye muscle spindles in man. Presumably these are the end organs for proprioception in the eye muscles. Similar end organs in an animal, the goat, have been studied by electrophysiological recording methods. The afferent discharges from these organs show an immediate increase in the frequency followed by a slow adaptation when a steady tension is applied (5). In a study by Hunt & Kuffler (7) the small intrafusal fiber of the muscle spindle has been shown to produce small increments of tension which in turn increase the rate of discharge of the afferent ending.

The intriguing possibility of recording nerve action potentials without surgery from the surface of the skin has been realized in the case of the more superficially located cutaneous nerve trunks (8). However, the limitations of this method make it questionable whether the technique will contribute much to the understanding of cutaneous sensitivity. Synchronized volleys in the median or ulnar nerves of man were elicited by electrical stimuli delivered through the skin to the nerve in the wrist without exposing the nerve. A nerve action potential of from 10 to 60 microvolts could be recorded at the elbow. Motor as well as sensory nerves contributed to the potential, although it was possible to obtain a small response with a stimulus intensity which failed to elicit a contraction of the muscles of the hand. A sensory effect was then localized in the region of the peripheral distribution of the stimulated nerve. This method might find its most likely application in the study of certain peripheral nerve injuries and lesions.

Direct stimulation of a peripheral nerve trunk through the skin by alternating current has been effected by Thompson and his coworkers (9). Such stimulation induces changes in the threshold for the adequate stimulus in the skin area supplied by the stimulated nerve. The greater change in the thresholds for pressure than for touch is taken as evidence that pressure sensitivity is mediated through the cutaneous nerves by temporal patterning of the impulses at a low frequency. Touch sensitivity depends upon the arousal of impulses in the same nerves but patterning is not important.

A new kind of problem in the study of vibratory sensitivity was subjected to exploratory study. Specifically, the task set by Smith, Sweet & Bartlett (10) was to discriminate phase differences between two vibrating dowels when the first and middle fingers, respectively, were placed on the ends of the two dowels. Subjects could discriminate a difference of 180 degrees with frequencies up to 830 per min. Threshold phase differences, when converted into time differences, seemed to yield a value around .01 sec., regardless of frequency, although the psychometric function appeared somewhat steeper for the higher frequencies.

One study with clinical orientation, made by Toomey, Kopency & Mickey (11), purports to demonstrate that the "Bioesthesiometer" could be used for the study of vibratory sensitivity. Administration of acetylsalicylic acid raised the threshold for perceiving vibration in 66 per cent of subjects tested. A comparative study of vibratory sensitivity was effected by training birds to

react to vibrations of the perch following extirpation of the auditory sense organs. The range from 100 to 3,200 cycles was studied. Maximum sensitivity occurred at around 800 cycles (12).

Central mechanisms.—The electrophysiological method has been applied to the analysis of the spinal and higher neural pathways. Conduction of muscle afferent impulses in the dorsal column and their relay through Clarke's column have been followed by Lloyd & McIntyre (13). Visceral afferent fibers in the splanchnic nerve, many of which represent discharges from Pacinian corpuscles of the viscera, were also found to ascend the spinal cord in the dorsal columns at a conduction velocity of 45 m.p.s. Cortically these discharges were represented in the trunk sensory areas. Discharges in the anterolateral columns were also recorded by stimulating still other visceral afferents of the splanchnic nerve (14). Rapidly conducted impulse components presumably from touch receptors were followed in the spinothalamic tract and medial lemniscus in the cat and monkey following stimulation of the sciatic and saphenous nerves (15).

Gay & Gellhorn (16) have mapped the proprioceptive cortical areas utilizing a number of different stimulating techniques. In the cat's cortex, the representation was quite widespread except for the extreme poles. Greatest concentration of activity occurred in the sensorimotor area. In the monkey, on the other hand, the cortical responses were largely restricted to the precentral motor cortex. Since the proprioceptive representation from the limbs is found largely in the motor area, it is believed by Gellhorn (17) that the proprioceptive return affects directly the excitability and function of the motor area. In fact, he has shown that the electrical excitability of the exposed cortex is directly dependent upon the degree of muscular tension in the peripheral muscles. Thus, the greater the tension upon a muscle, the greater the response of that muscle following cortical stimulation. These findings are of interest to the old problem of maze learning on the basis of kinesthetic cues. If proprioception is so intimately related to the motor areas, it would be impossible to dissociate motor and proprioceptive function by the commonly employed cortical ablation techniques. It might be said that proprioception and excitability of the motor cortex are coexisting functions. Without an adequate proprioceptive return the limbs become ataxic, for example, as in the case of dorsal root section. Under these conditions, muscular contraction is possible but voluntary movements are not initiated. Further studies of direct electrical stimulation of the exposed human brain have been reported by Penfield & Welch (18). In this particular communication they stress the instability of the sensory points and record instances of facilitation and sensory inhibition.

Pain.—Since the development of the Hardy-Wolff-Goodell radiant energy pain stimulator, there has been increased interest in the study of pain. Another, less precise pain stimulator making use of a heated stylus has been described by Campbell (19). A theoretical treatment of the Time and Intensity relations in radiant energy pain stimulation has appeared (20). The Hardy-Wolff-Goodell device has been employed to study possible changes in

pain sensitivity associated with diseases of the skin (21). Tissue damage resulting from dermatosis was found to be an infrequent cause of altered pain threshold. There was no correlation between itching and altered pain sensitivity. This same method has been employed by Hardy & Javert (22) in the effort to quantify pain as experienced during childbirth. The standardized pain stimulus was applied to an area of the hand during the various states of labor and following childbirth. The intensity of the cutaneous pain was adjusted until judged equal in intensity to that experienced during the labor contractions. No analgesic or anesthetic was administered to the subjects who volunteered for the study. The progressive increase in the pain intensity was roughly proportional to the dilation of the cervix during the first stage of labor. During the second stage maximal pain was experienced. In the fourth stage the pain was much reduced and approached threshold values in about two hours after delivery.

Differences in pain reactions were found in patients with peptic ulcer and patients showing the menopausal syndrome. Both patient groups had significantly lowered pain reaction thresholds, although the pain perception threshold was not reduced. Whereas the pain perception threshold is the lowest intensity for producing a definite, sharp stinging sensation, the reaction threshold is that intensity at which the subject winces or draws away (23, 24). The greater lability of a pain reaction threshold was shown in an experimental study wherein the galvanic skin response was recorded following the application of pain. Although the pain perception threshold remained constant, the galvanic response, as might be expected, showed adaptation upon repeated stimulation. In any one series the magnitude of the response was proportional to the intensity of the stimulus (25).

The work of Weddell and his group (26) on the neuro-anatomical factors in pain has continued. The difference between the character of deep and cutaneous pain is attributed in part to differences in the density of innervation of these two areas. Pain referred to a cutaneous area from irritation or injury to some visceral structure, according to these workers, can best be accounted for by branching of the fibers conveying pain impulses. Thus, two branches of the same nerve cell supply respectively the deep visceral structure and the cutaneous area to which the pain is referred. Impulses initiated at the site of the visceral disturbance arrive at the cutaneous area by antidromic discharge. Axon branching, thus, could lead to a mislocation of the origin of the impulses and secondarily to a release of metabolites in the referred area which, in turn, might initiate pain impulses at the cutaneous site (27). Injection of local anesthetics or physiological saline at the site of such referred pain is known to bring relief in some instances. If the intensity of the stimulus for the visceral pain is too strong, the peripheral injection will not eliminate the pain (28). The intensity of pain aroused by distension of the intestine studies in patients with intestinal fistulas was found to be proportional to the degree of distension (29). A description of the intensity and distribution of pain produced by stimulating the nasal mucosa has been included in a recent monograph (30).

Sinclair (31) has suggested that the referral of pain from the viscera is only a special case. Referred pain can also be demonstrated when the primary site of irritation is the skin itself. A number of well-defined "trigger points" in the skin of normal subjects elicited pain referred to other cutaneous areas, sometimes at a considerable distance. Prolonged stimulation might lead to alterations of cutaneous sensibility in the area of reference. A direct physiological study of mechanisms of this sort has been carried out by Hapgood (32) with the frog skin preparation. When a weight is dropped on the normal skin, a discharge of impulses occurs at the moment of application. This is followed by a discharge of a few impulses as long as the weight rests on the skin. The magnitude of this response may be much increased by local injury produced by scraping ("sensitization"). Sensitization also occurs after antidromic stimulation of the cutaneous nerve to the skin. Antidromic impulses down one nerve which overlaps the area supplied by a second nerve will produce dromically conducted impulses in the second nerve. This effect appears to be due to the liberation of a chemical in the skin. The same effect can be achieved when two separate pieces of skin are placed on top of each other with the epithelial surfaces contiguous. Stimulation of the afferent nerve of the upper piece will produce discharges in the nerve from the lower piece of skin. The parallel between hyperalgesia and referred pain in humans and the behavior of the frog skin preparation is quite clear.

Central mechanisms have been stressed in several studies in which the spread and summation of pain does not appear to follow the peripheral nerve course. Immersing a finger in water at 0° C. produces pain which overflows to neighboring digits primarily in a caudad direction (33). The spatial summation of pain noted when a pin or needle point is dragged across the skin is also interpreted in terms of a process in the spinal cord (34). Wolff, Hardy & Goodell (35) have investigated the mechanism of hyperalgesia induced in areas adjacent to areas of noxious stimulation. In these areas there is no elevation of the threshold, although the reaction to suprathreshold stimuli is more intense and prolonged. They interpret this as central summation produced by the continued barrage of impulses from the affected area.

A general discussion and review of causalgia by Echlin, Owens & Wells (36) points out that various proposed mechanisms may all contribute to the picture as seen clinically. It has been stated that the causalgic condition is due to a vicious cycle of activity originating in nerve impulses from an injured peripheral nerve. This, in turn, creates an abnormal state of activity in the internuncial neuron centers of the gray matter of the spinal cord. From this arise efferent discharges which are responsible for the muscular and vasomotor spasms often seen. If the process is allowed to continue, the abnormal activity of the spinal cord may become self-sustaining, ultimately to affect the higher centers. "Cross-talk" between the efferent and afferent fibers may be involved and the antidromic activity of the afferent fibers thus initiated may release a chemical agent in the skin to produce sensitization. Evidence exists to some extent for all these mechanisms.

A further attempt has been made by Benjamin & Ivy (37) to determine a

cortical effect or cortical representation for pain on the basis of changes in the human EEG (electroencephalograph) following cutaneous stimulation by v. Frey hairs for pain. Changes in the EEG were observed in the parietal, occipital, temporal, and frontal areas in decreasing order of effect but the change was not specific for pain. The same changes could be induced by temperature and touch stimuli.

Sensory "extinction."—The phenomenon of sensory "extinction," "suppression," or "inattention" following parietal damage continues to claim the attention of workers in the clinical field. This phenomenon is found when bilaterally symmetrical stimuli are applied to the body surface. The stimulus contralateral to the brain damage is not perceived, although, when that same stimulus is presented alone, it is perceptible. Several general reviews and discussions of possible mechanisms have appeared (38, 39, 40). In one case described by Henson (41) it was found that unilateral hypersensitivity to pain, typical of thalamic involvement, could be suppressed upon the application of identical bilateral stimulation. At one stage of the illness, the severe pain on one side of the body was suppressed by the application of hot water bottles to the normal side.

A careful psychophysical experiment on weight lifting was carried out by Teuber *et al.* (42) with 16 men on whom medical, surgical, and x-ray evidence showed loss of brain substance in the parietal region. In the method of constant stimuli employed, the standard was held in one hand and the comparison stimuli in the other. The weight of the hand contralateral to the lesion was consistently underestimated. Yet, when the standard and comparison weights were hefted with the same hand on either the normal or affected sides, there was no impairment of discrimination. This occurred even when the two-point discrimination was impaired as a result of the parietal lesion. This approach to the problem of "extinction" is of interest to the experimentalist because of possible relations to the classical time error problem in psychophysical judgment.

Olfaction

Studies in the field of olfaction may be grouped under two main categories: (*a*) studies of afferent neural mechanisms, especially anatomical details, and (*b*) studies of sensitivity with particular regard to the nature of the stimulation process.

Afferent neural mechanisms.—The quantitative relations among the sensory epithelium, olfactory bulb, and olfactory tract have been analyzed by histological methods (43). In the three to four months' old rabbit preparation, there are 50,000,000 receptor cells in each nasal epithelium which, in the olfactory bulb, converge upon some 1,900 glomeruli or dendrite clusters. Each glomerulus, therefore, receives impulses from something like 26,000 receptors and relays them in turn through 24 mitral cells and 68 tufted cells. It is the former group that gives rise predominantly to the axons of the olfactory tract. This clearly indicates the degree of convergence said to be responsible, at least in part, for the great acuity of the sense of smell.

The central connections of the olfactory nerves have been subjected to further examination, and evidence is accumulating to the effect that much of the rhinencephalon, as described in standard works on neuroanatomy, does not have a demonstrable olfactory function. Smith (44) has shown on the basis of excitation and ablation experiments that the piriform-amygdaloid-hippocampal complex participates in various ways in the general behavior of the organism and appears to have transcended its role in the olfactory system. The further delimitation of the terminations of the lateral olfactory tract has been extended to the monkey by Meyer & Allison (45). Following section of the lateral olfactory tract, areas of degeneration were found in the dorsal surface of the temporal lobe in the superficial layer of the prepiriform cortex forming the anterior part of the hippocampal gyrus. Some fibers terminate on the orbital surface of the frontal lobe in the olfactory tubercle and in the peri-amygdaloid area. No olfactory fibers could be traced to any part of the hippocampal formation in the lateral ventricle. Further histological studies of the olfactory system have been carried out during this period (46, 47).

Studies of sensitivity.—The problem of olfactory classification has been re-examined by Ross & Harriman (48) specifically in the case of the Crocker-Henderson system. Untrained subjects, when asked to arrange the 32 standard Crocker-Henderson odors in as many groups as necessary, varied from 4 to 19 groups in their responses. The Crocker-Henderson system has four groups: acid, burnt, fragrant, and caprylic. In a second part of the experiment, 15 untrained subjects were presented with an assortment of each of the four categories of standard odors. They were instructed to rank them in order of each of the four basic qualities. Each quality was named and illustrated by the strongest of each series. According to the authors, little correspondence with the standard arrangement for intensity was obtained. They concluded that the Crocker-Henderson system is inadequate. Casual inspection of their data indicated to the present reviewer that there was some consistency to the ranking made for each of the four basic categories. Their results for the acid series rescored in Table I show the number of times each of the eight standard odors (1 to 8) was judged to be in each of the categories from 1 to 8 (degrees of subjective intensity). There appears to be a trend in the direction specified by Crocker and Henderson, although this falls below the order of accuracy claimed for the method.

A series of articles by the French workers has emphasized the apparent multiplicity of the sensory mosaic of the olfactory end organ in an effort to make a chemical interpretation of olfactory stimulation. The well-known phenomenon of selective olfactory adaptation has been utilized by LeMagnen (49) in studying chemically different substances producing the same or similar odors. For example, an almond-quality odor is produced by both benzaldehyde and benzonitrile. Adaptation to the latter for a ten-minute period not only reduces sensitivity to the benzaldehyde but also changes the quality to resemble an odor like that of safrole. Combined adaptation to both benzonitrile and safrole uncovers still another residual which has a

TABLE I

INTENSITY RANKING OF 15 OBSERVERS FOR THE EIGHT STANDARD ODORS IN THE ACID GROUP. INTENSITY RANKED FROM 1 (LOW) TO 8 (HIGH)

Eight standard odors	Ranks assigned by 15 observers								Mean rank assigned to each
	1	2	3	4	5	6	7	8	
No. 8	0	0	0	3	0	1	7	4*	6.6
No. 7	0	0	2	4	0	0	6*	3	5.9
No. 6	0	0	5	7	0	1*	1	1	4.3
No. 5	0	0	7	1	2*	0	0	5	5.0
No. 4	3	1	0	0*	3	6	1	1	4.7
No. 3	4	1	1*	0	3	5	0	1	4.1
No. 2	5	3*	0	0	5	2	0	0	3.2
No. 1	3*	10	0	0	2	0	0	0	2.2

quality like indole. This author believes that odorous qualities depend upon underlying receptor sensitivities and that a multiplicity of such receptors are stimulated by most odors. Different substances which produce the same odor apparently stimulate different receptor cells, since they undergo a different sequence of qualitative changes during adaptation. It is believed, therefore, that it will be impossible to explain the similarity of odor quality on the basis of some common physical property like the Raaman spectrum.

Further evidence on the nature of these receptors is presented by Guillot (50, 51) from the study of partial anosmias which occur frequently in otherwise normal subjects. Of particular interest is the differential anosmia for two groups of substances which have the same odor, exaltolide and other macrocyclic musks on the one hand and substances resembling musk ambrette on the other. In a number of instances, partial anosmia was found for either group alone and in a few instances for both together. In the latter case, sensitivity for a third substance, nitro-musk, was normal. Guillot believes that the study of such partial anosmias will yield some indication of the basic odor receptors. These may number as many as several hundred. This hypothesis stands in opposition to any schema which reduces to a few fundamental odors as in the Crocker and Henderson classification. The possibility of a morphological as well as chemical differentiation among olfactory receptors (51) is also advanced. Another group of workers also argue for a physico-chemical mechanism of olfactory stimulation, since odor quality and intensity are not related in any simple way to the molecular or physical properties of odorants (52).

This kind of reasoning, of course, is in opposition to the search for some common physical property in chemically different substances that elicit the same odor. The search for this property is, in part, the orientation of the recent work of Miles & Beck (53) on the infrared absorbtion spectrum of odorous substances. According to these workers, the initial physical event

in the stimulation of the olfactory receptors is due to an increase in their heat radiation caused by the heat-absorbing properties of certain gases. One recent experiment was carried out on honey bees by using two airtight chambers containing honey vapor. Both chambers contained infrared passing windows, but one was blocked by the insertion of a glass window on its inner surface. In field tests bees were first attracted to the devices by honey. After this was consumed, it was noted that more bees collected in front of and around the infrared passing window than around the control. These results were taken to support the view that the behavioral difference was due to increased heat losses from the insects' antennae.

Another study has been carried out by Miles (54), using cockroaches. When a tray containing these animals was suspended over a homogeneous surface in the dark they showed no tendency to cluster in any area of the cage. When a checkerboard pattern was placed beneath the cage, 15 per cent more animals tended to cluster over the black than over the bright squares. The probability is less than one in 1,000 that similar results could be achieved by chance. Various rotations and rearrangements of controls were instituted. As these experiments stand, there is evidence for an effect of loss of radiation upon the behavior of bees and cockroaches. However, as pointed out by Patton (55), these experiments can be interpreted in line with the radiation theory of olfaction. They do not preclude other interpretations, especially in the latter experiment where olfactory sensitivity is not necessarily responsible for the orientation over heat-absorbing surfaces.

The idea that internal changes in the body may influence olfactory sensitivity has occurred from time to time. This notion is attractive because a chemical sense might be expected to respond to chemical changes of the body fluids in which they are embedded. LeMagnen (56) has noted that an anosmia for exaltolide, the synthetic form of the active component of musk, is sex linked and occurs quite regularly in man. This occurs rarely in women, especially after puberty, but the sensitivity in women varies during the menstrual cycle. This effect was thought to depend upon the appearance of the sex hormones in the blood stream. In order to check, by a direct study, the possibility that chemical agents in the body may sensitize the olfactory endings, several odorous substances were injected (57). After a one- to eight-day period, the threshold for the injected substance fell below the value existing prior to the injection. A gradual desensitization then set in. This study was followed by another on the effect of androgens and estrogens upon olfactory sensitivity in man (58). Androgen led to an elevation of the threshold for pyridine (which has a urinoid odor) but a fall for safrole. Estradiol increased the sensitivity for pyridine but reduced the sensitivity for safrole, indicating some sort of reciprocal relation between these two odorous substances. The results of these experiments are presented in abstract form and unfortunately there are no data available for the inspection of the reader. These results are certainly interesting, but as in all cases of threshold studies, it is desirable to know the amount of the change so that appropriate statistical procedures may be employed.

Changes in olfactory sensitivity reported by workers in this country

using the Elsberg technique following the ingestion of sugar or other agents are hard to assess because of the method of stimulation (59). The Elsberg technique has the great advantage of simplicity. However, there is evidence that the threshold obtained by this method is much influenced by factors in transport of the odorous vapor to the olfactory epithelium. Changes in the state of the nasal mucosa, as described by Holmes *et al.* (30), might be expected to influence the threshold. Such a change would have only a superficial relation to olfactory sensitivity, per se.

A new book, *Odors: Physiology and Control* (60), is noteworthy because of the extensive bibliography of some 124 pages. The earlier sections of the book present the anatomy and physiology of olfaction. The anatomy section contains several good plates showing the extensive olfactory bulb of a macrosmatic animal. The physiology section shows an intriguing photograph of a rat prepared for recording electrical activity in the olfactory nerve (courtesy of the Florida Chemical Research, Inc.). One would like to know more about this line of investigation, but no mention of it is made in the text. A goodly section of the remainder of the book deals with certain practical problems in relation to food, industrial odors, ventilation, water supplies etc.

Taste

During the past year, studies concerned with taste have fallen in the following main categories: (*a*) studies of afferent-neural mechanisms in taste, (*b*) studies of thresholds and psychophysical relations, including taste blindness, and (*c*) the relation of taste to dietary self-selection.

Afferent-neural mechanisms.—Two studies on the discharges in the glossopharyngeal nerve of the frog (61, 62) showed that certain chemical stimuli like quinine and mild acid produced impulses of relatively small magnitude as compared with those elicited by touch. On the other hand, a rather remarkable sensitivity to water and hypotonic solutions was found for receptors connected with fibers giving off large potential spikes. Hypertonic salt solutions also elicited this same type of discharge, whereas isotonic solutions were ineffective. That this is not simply an undifferentiated response of tactile endings to osmotic pressure differences is indicated from observation of single fiber preparations. Fibers connected to water-sensitive endings failed to respond to tactile stimulation. In a study of the same nerve in the toad, Andrew (63) found a similar effect, but apparently was unable to obtain responses to other chemical solutions, for he concludes that the chemoreceptors in this animal appear to be of a single type responding to hypertonic solutions and in some instances to hypotonic solutions. The latter would seem to be a response due to the "water receptor" of Zotterman (61).

The frog preparations were found to be unresponsive to sugar, a finding which agrees with the previous observations on the cat and with more recent studies of the chorda tympani of the rat by the present reviewer (64). The discharge in the rat preparation is characterized by a very pronounced discharge to sodium chloride, to other salts, and to acids. Thresholds for both sodium chloride and acid were readily obtained by noting the concentration which just elicited a discharge of impulses in the nerve. Since the threshold

seemed to vary from one application to the next, a modified method of constant stimuli was employed, in which series of stimuli were dropped on the tongue in random order for at least ten stimulations per concentration. The presence or absence of a response in the nerve was judged by the appearance of the oscilloscope and the sound of the discharge in the loudspeaker of the recording system. The frequency of occurrence of a response was an increasing ogival function of the stimulus concentration. A threshold was then computed from the midpoint of this curve. In some preparations where water produced some activity, a correction was made for this factor. The average thresholds for a group of normal animals were as follows: sodium chloride, .009 per cent; hydrochloric acid, .001*N*. Sugar produces almost no effect. In an occasional preparation, it appeared that some slight activity occurred after an extremely long latency. Somewhat more activity following quinine stimulation could be detected in the more optimal preparations. The consistent failure to record a definite response to sugar in the gustatory nerves in several different species suggests that the nerve fiber system conveying impulses for sweet may be of relatively smaller diameter, or even unmyelinated, so that their action potentials are not easily recordable above the noise level of the conventional amplifier. In any case, the clarification of the afferent neural mechanism for the sweet taste is needed, for it is quite clear from behavioral studies of taste preferences in the rat that this animal can discriminate sugar, saccharin, and other similar substances.

The cortical representation of taste has been studied by Patton & Amassian (65) by the evoked potential method following single shock stimulation of the chorda tympani nerve. A surface positive wave of from 50 to 200 microvolts in the cortex of the cat was found in a 4 to 5 sq. mm. area on the rostral orbital surface superior to the rhinal fissure and anterior to the anterior ectosylvain fissure. It lay rostral to the tactile response somesthetic zone for the face. Patton points out that the chorda tympani contains some tactile fibers, but that the majority of the afferent fibers in this nerve are gustatory in function. The cortical responses were abolished by transtympanic destruction of the chorda tympani. The possibility of surgical interference with this particular peripheral nerve pathway in humans is discussed by Dübendorger (66) in an article concerned with the incidence of taste disturbances following the fenestration operations.

Psychophysical studies.—The third article of a war-interrupted series by Hahn and his associates (67) has appeared. This investigator has utilized the Hahn stimulator or "taste microscope," a U-shaped glass tube containing an aperture against which the subject holds his tongue as taste solutions flow through the apparatus. In this particular study, mixtures of substances which produce the same and different tastes were studied. The threshold for all 108 substances was either decreased or remained unaltered by admixture of a second substance. A decrease in the threshold was detected only when the second substance had the same taste quality as the first substance. Unfortunately, only a smattering of data is presented in the form of one or two illustrative experiments. A summary table showing the results simply as "change" or "no change" in threshold for the different subjects and different

substances contain what appear to be exceptions to the conclusion reached by these authors.

A study of the thresholds for a number of toxic metallic salts and other impurities was determined in pure aqueous solutions, as well as in the presence of other tastes, as sweet, salty, and bitter (68). In the latter case, of course, with these substances some masking occurred. This author points out that chemical analysis frequently is not satisfactory for the assessment of foodstuffs. Some form of psychophysical procedure is required (69).

A detailed review of the methods of assessing food and beverages is contained in an article by Boggs & Hanson (70). Here, the emphasis is primarily upon practical considerations in relation to the food industry. A distinction is made by the authors between tests in which some specific quality as taste, rancidity, or texture is judged and preference tests in which likes and dislikes are stated. Of particular interest is the statement that "solutions of pure chemicals have been used for many years to demonstrate the differences in sensitivity of individuals to sweet, salty, sour, and bitter. However, except in extreme cases, the results of such tests have not been of much value in predicting sensitivity to food." Presumably, the discrimination of some characteristic of food such as saltiness is less determined by thresholds of the gustatory receptors than by the ability to abstract and to differentiate some one quality in the presence of a wide variety of other qualities.

The fact that data obtained in relatively simple psychophysical situations are not simply related to tasting of foods and beverages also stems from Beebe-Center's use of the gust scale with appropriate standards (71). He utilized solutions of sucrose, quinine sulfate, tartaric acid, and sodium chloride, corresponding to the four basic qualities. These were arranged in a rack of four columns by nine rows, the latter providing nine concentrations separated by quarter log gusts. The observer reported a match for any particular foodstuff with respect to a given quality. His judgment was made in terms of the standard which best matched the taste of the food, as say, "bitter 7." This scale seemed to be reasonably reliable and a series of specifications of different foodstuffs was obtained. The subjective strengths of the single taste qualities of acceptable foodstuffs rarely exceeded 50 gusts and tended to center around 10 gusts. The author concludes that on the whole, acceptable foods are rather bland. So far as acceptance is concerned, sweetness did not appear to have any particular advantage over bitter or sour. It is this finding which is at variance with the work based on the simple situation where quinine sulfate was judged to be predominantly unpleasant for concentration of .0011 per cent and higher. This represents an intensity of five gusts or more. Actually, a very successful ale was found to have a psychological bitter component of 28 gusts and a normal brew of black coffee one of 23 gusts even when sweetened in the normal manner.

A series of short notes dealing with the identification of cola beverages has appeared which indicates that subjects cannot distinguish among the several commercial varieties when they are presented in a randomized fashion (72, 73, 74).

The puzzle of taste blindness continues to evoke interest. The incidence

of this effect among the Japanese (75) appears to be somewhat less than among white persons. A couple of studies concerned with the chemical and physiological bases for the phenomenon have appeared. Harris (76) investigated a number of substances, phenylthiourea, symmetrical diphenylurea, thiourea, thioglyoxaline, and thiouracil and found a high correlation for taste sensitivity to all these substances. There was no relation between sensitivity for these substances and urea, phenyl urea, and uracil. Cohen & Ogdon (77) have performed a significant experiment on the importance of saliva for the ability to taste phenylthiocarbamide. They tested the hypothesis that some characteristic of the saliva of tasters differed from that of nontasters. The tongues of their subjects were thoroughly dried. The substance was then presented in a solution of the saliva, either from tasters or nontasters. Tasters could taste the phenylthiocarbamide only when presented as a solution of their own saliva. The substance could not be tasted in anyone else's saliva even if the donor were a taster. There was some indication that the taste for other substances like saccharin would be affected in a similar manner. A review article by these same authors on the subject of taste blindness has appeared (78).

Specific hungers.—The study of the specific hungers and food preferences of the rat as determined by taste continues to be an active area. The relation of preferential behavior to the intensity of the gustatory stimulus (concentration of salt solution) is described in three papers (79, 80, 81). The preference for sodium chloride solutions over water in normal laboratory rats is directly proportional to the concentration of the solution from the preference threshold value to the peak preference or optimally preferred solution. The preference threshold has been variously found to lie between slightly less than .01 per cent to .06 per cent with the peak preference falling from .5 per cent to approximately 1.0 per cent, depending upon conditions of testing. Beyond this value, the intake of salt solution decreases, until at 3.0 per cent and above, almost no salt solution is taken. In one of these studies the increased salt preference found by other workers following adrenalectomy was confirmed. But in another, adrenalectomy produced only slight effects upon behavior (82). Assuming that the operations were successful, it is difficult to understand why adrenalectomy had such little effect. The effect in Bare's study (79) was very pronounced. It is of particular interest that the salt preference was increased for all solutions, weak as well as strong, and that there was no particular shift in the peak preference. In other words, the function relating salt intake to strength of the stimulus was essentially the same in normal and adrenalectomized animals, except that the whole curve was elevated. Adrenalectomized animals, therefore, showed a lowered preference threshold. An effort was made to determine whether this change in preference could be attributed to a change in the sensitivity of the taste buds as suggested by Richter (88). The electrophysiological method was used to determine the gustatory thresholds for salt in normal and adrenalectomized animals. The adrenalectomized animals had thresholds in the same range of values as did the normal. These values fell below the preference value usually obtained in the normal animal, but approximated

the preference threshold of the adrenalectomized animal. It was concluded that no change in gustatory sensitivity occurs in this particular condition of salt need. Further, it would appear that the normal animal can taste the salt in weak concentrations but does not take it. The adrenalectomized animal does (82).

Three other studies utilizing changes in self-selection pattern following physiological manipulations of the organism have been carried out. In that by Munan & Albritton (83) the difference between two groups of rats was attributed to a difference in strain. Experimentally-induced hypertension in rats and rabbits leads to a lowered intake of sodium salts, but to no change in the intake of certain other salts supposed to have a beneficial effect upon this condition in man (84). Since salt metabolism is intimately related to water balance, an effort was made to demonstrate a change in the salt preferences due to water deprivation. Water deprivation produced a marked change in the preferential behavior; thirsty animals showed no preference for salt, only the avoidance of the stronger solutions. This appeared to result largely from the indiscriminate drinking of the weaker, but not the stronger, salt solutions (85).

The appetite for thiamine in solid foods has been further investigated. In the ninth paper of a series on the self-selection of diet, Scott & Verney (86) showed that an appetite for thiamine was not universally present in rats deficient in this respect. Further, rats fed a diet with a critically low concentration of thiamine were in better condition than those offered a choice of this same diet and vitamin-free food. Although deprived animals tend to make a compensatory choice in favor of the needed material, they would have thrived better on an obligatory diet, as judged by weight curves.

Young (87) has recently reviewed the vast array of data from his own and others' experiments on food-seeking behavior. He points out that food-seeking behavior depends upon three factors: (*a*) habit, (*b*) the chemical state of the organism, and (*c*) the characteristics of the foodstuff or palatability.

Although food selections often are in accord with nutritional needs, the correlation between need and acceptance is far from perfect. Food acceptance is regulated by the characteristics of the food object (palatability), by the environmental surroundings of the food object, by established feeding habits, as well as by intra-organic chemical conditions which themselves may or may not be directly related to metabolic needs.

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INDIVIDUAL DIFFERENCES[1]

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The publication of four new textbooks in the general field of mental testing makes information about all types of psychological measuring instruments more readily available than it has been heretofore. These books by Mursell (1), Cronbach (2), Goodenough (3), and Freeman (4) include discussions of the projective techniques as well as the traditional types of tests, and all place considerable emphasis on the interpretation of test results. Mursell, Goodenough, and Freeman have chapters which summarize the principal research findings in individual differences. A new French text in differential psychology by Piéron (5) is also available.

New Tests and Techniques

Lundin (6) has developed a new set of music tests based on musical tasks more complex than those called for by the Seashore battery. They include such performances as interval discrimination, melodic transposition, mode discrimination, melodic sequences, and rhythmic sequences. He reports reliabilities of .85 or higher and a correlation of .69 with theory grades. It is not clear, however, that the validity coefficient would be that high had the students taken the test before undergoing the theory training. These tests illustrate one of the main trends in recent work in that they make no pretense of identifying native, unlearned talents.

Listening ability, or the ability to comprehend orally presented verbal material, has been investigated by Brown (7). His test includes such materials as oral directions, grasping word meanings through contextual cues, getting details and central ideas, etc. The fact that scores on the test show only low correlations with intelligence, reading comprehension, and high school achievement would seem to indicate that some new variable is represented in the listening situation, but its nature is not apparent.

An important series of studies by Witkin (8, 9) and by Witkin & Wapner (10) has been concerned with the way in which people perceive the upright position when visual and kinesthetic cues are varied. They find that there are marked individual differences in the ability to maintain correct spatial orien-

[1] The period covered by this review extends from June, 1949 through May, 1950, but a few papers with earlier dates have been included if they seemed important and had not been previously reviewed. There are undoubtedly a number of omissions. Not quite all of the British and almost none of the Continental publications were available to the author. On the whole, the coverage is probably fairly adequate for the British and American work, but does not extend beyond it.

tation. That some general trait is involved is indicated by the consistency of scores from one test situation to another and their stability over a period of a year. This ability is also significantly related to performance on a visual perception test involving the recognition of hidden figures. Work is in progress on personality characteristics related to these perceptual differences.

Undoubtedly one of the most important publications of the year is the book by Adorno *et al.* (11) which reports in detail on a large-scale research project concerned with ethnocentrism and the personality traits which underlie it. While its major implications are for social psychology, it makes a highly significant contribution to the field of individual differences in its delineation of a personality syndrome which may form the basis for a more meaningful description of individuals than has heretofore been possible. A number of new measuring instruments were devised during the course of the reseach: an anti-Semitism scale, an ethnocentrism scale, a political-economic conservatism scale, and finally a more general measure of prefascist tendencies, the F scale. It is this last set of items which will be of most value in subsequent personality research, as they seem to get at the core of conventionalism, authoritarian submission and aggression, anti-intraception, superstition and stereotypy, power and "toughness," destructiveness and cynicism, projectivity, and concern with overt sexuality to which prejudiced attitudes of all sorts are attached. From the standpoint of methodology, the combination of clinical and statistical investigation is worthy of note.

In a related study, Frenkel-Brunswik (12) has found that a relatively simple perceptual variable, the inability to tolerate ambiguity in a perceptual situation, is also related to prejudiced social attitudes. In the theoretical discussion, she shows that this variable may have much in common with typologies previously proposed by Jaensch, Kretschmer, Goldstein, and others.

A new application of factor analysis methods to personality research has been proposed by Cattell & Luborsky (13). In using this P-technique, they start with an original matrix of correlations between repeated measurements of the same variables in the same individual. The factors thus turn out to represent things which show the same fluctuations over a period of time and bring together both physiological and psychological variations. Four of the nine factors can be identified with those that have come out of Cattell's previous R-technique research.

Clarification of Existing Techniques and Hypotheses

The *Psychological Bulletin* has performed a service by bringing together critical reviews of all 19 volumes of the Army Air Forces Research (14). Investigators wishing to make use of any of these methods or materials can readily ascertain from these reviews which volume is likely to contain the materials they want and what its limitations are. A special study growing out of the AAF program has been reported by Michael (15). He has made separate factor analyses for two groups differing quite markedly in ability and background, West Point cadets and Negro pilot trainees. By including

the correlations with the pass-fail criterion in the original matrix, he was able to evaluate the extent to which each of the different factors obtained entered into the criterion. While the factor patterns for the two populations are similar and resemble quite closely the results obtained with other Air Force groups, the contributions that the factors make to the criterion differ considerably in the two cases. Such results point to the need for caution in using factor scores for selection and guidance purposes unless what they mean in the particular kind of group to which they are being applied has been investigated.

Several other studies have indicated that, so far as school achievement is concerned, common-sense inferences from factor scores may not necessarily be sound. Shaw (16) reports results which call attention to the limitations of the Primary Mental Abilities Tests for predicting high school achievement as measured by standardized achievement tests. The highest correlations with specific subject-matter areas, ranging between .4 and .8 are with *V* (verbal). There are some moderately high correlations with *R* (reasoning), only a few that are significant with *N* (number), and almost none for *W* (word fluency), *S* (space) and *M* (memory). Furthermore, relationships are not always in the expected directions. Scores on *N*, for instance, correlate highest with writing correctness (.43) and only .19 with science and .32 with quantitative thinking. Murray (17) also cites some evidence that relationships are not always what one would expect to find. Scores for spatial and reasoning abilities contributed less toward success in high school geometry than did numerical and verbal abilities measured by Modified Alpha Examination, Form 9. One of the most comprehensive studies so far on the prediction of school achievement from specialized tests is the research report on the Differential Aptitude Tests issued by Bennett, Seashore & Wesman (18). The correlations they report are, on the whole, in the expected directions, but there is a strong tendency for scores on all of the tests to be related to success in all of the courses.

Some factor analysis studies have been concerned with clarification or more precise description of factors often reported before. Swineford (19) maintains that the number factor identified in her work is related to the set or attitude which pupils have toward numbers. This relationship appears to be more marked for girls than for boys. In another paper, Swineford (20) analyzes the results obtained when a group of 88 pupils took the same test battery in the sixth and in the ninth grades. The three bi-factors previously reported—general, verbal, and spatial—appear in both age-levels and there are substantial correlations (.3 to .8) between factor scores on the two occasions. Students show the greatest increase over the three-year period in *G*. No increase at all is shown in *S*. Some change occurs in the weights which specific tests have for predicting factor scores, particularly with regard to *S*.

Some attention is still being given to the question of what intelligence tests measure. Jones (21), applying rotational procedures to the factor loadings obtained by McNemar (89) from Terman and Merrill's standardization

data (90), presents evidence for four factors rather than the one general factor McNemar had postulated. Magaret & Thompson (22) supplement their previous paper on differences between normals and mental defectives of the same mental age by bringing in another comparison group of superior children of the same mental age (IQ above 120). An even more marked tendency appears for differences to occur on items most highly saturated with McNemar's general factor. In other words, whatever the trait is that makes for correlations between Binet subtests, it is the trait which best distinguishes between the bright, the normal, and the dull.

Tilton (23) takes issue with the conclusion several times stated in recent years that intelligence tests do not measure ability to learn. He presents data showing substantial correlations between IQ and gains on a history achievement test when the test is modified by the omission of easy items and items showing chance changes from initial to final score. Thus, this controversial issue is left open for further discussion. Perhaps the solution will be to break down the rather ambiguous concept "ability to learn" into two components, "amount" and "difficulty." We might expect measured intelligence to be related to the latter more decisively than it is to the former.

Some work has been done on motor abilities, corroborating, in general, previous conclusions that they are less general than cognitive abilities. Seashore, Dudek & Holtzman (24) have identified three separate factors making for correlations among steadiness tests. The first two, moderately correlated with each other, have to do with steadiness of movement in three-dimensional space and in a restricted plane. The third, largely independent of the other two, represents stationary steadiness or freedom from involuntary movement. Since the three factors taken together account for only about 50 per cent of the variance, they conclude that there may be other steadiness factors not well represented in the test battery they were using. Goodman (25) demonstrates a novel way of supplementing factor analysis by time and motion analysis in order to identify and describe the abilities involved in motor performances. In his study of the MacQuarrie test, a visual inspection factor which emerged from the mathematical factor analysis was verified in the time and motion study, and a manual movement factor identified by both techniques was named more precisely on the basis of the time and motion study than it could be from the factor analysis alone.

British factor analysts have characteristically interpreted their findings in a somewhat different manner from American workers. A paper by Vernon (26) throws some light on the nature of the trait they have been calling "practical ability." It brings together evidence from many heretofore unpublished studies, largely military selection projects, that after *g* has been accounted for, two main sub-types of ability can be differentiated. These he calls *v:ed* (verbal, educational, numerical) and *k:m* (practical, spatial, mechanical). Each of these can be further subdivided, but variances for such sub-factors are small. The data seem to indicate that *g* does not become a less significant determiner of variance with maturity, as many workers have thought. [An American study by Curtis (37), cited in a different con-

nection in the next section, corroborates this conclusion.] So far as prediction of success on specific practical jobs is concerned, their results show that general personality factors carry more weight than specific patterns of ability. These findings take on special practical significance since they seem to set definite limits to the usefulness of so-called aptitude tests in vocational guidance. Holliday (27) reports a rather loosely organized study in which apprentices and applicants for apprenticeship in an engineering firm were asked to rate themselves as to whether they were better on practical than on theoretical activities or the reverse. There seemed to be a fair amount of agreement between these self-judgments and the differences between scores made on the intelligence and mechanical aptitude tests, but the inadequate statistical treatment leaves us in doubt as to just how close the relationship is.

A number of studies have contributed to our understanding of what is being measured by vocational interest tests. Strong (28) has attacked the problem of what differences in score on the different "Accountant" scales of the Strong Vocational Interest Blank mean. They seem to be related to occupational level or attitudes toward responsibility and authority. Kriedt (29) has studied the Strong scores of four different types of psychologists and shown that separate interest patterns can be differentiated. His work has resulted in the construction of a new "Psychologist" key and four special keys for clinical, experimental, guidance, and industrial fields of specialization. Clark (30) has constructed a vocational interest test of the Strong variety at the skilled trades level and reports some evidence that various skilled trades can be differentiated as the professions are, when groups of workers are compared with others of the same level. Whether his keys would be effective with groups other than those from which they were derived has not been demonstrated. Previous research in this area would make us cautious about our conclusions until such evidence is presented. Mosier & Kuder (31) show that men in different occupations can be differentiated on the basis of preference scales for personal rather than occupational characteristics. The five preference scales they used have been derived by item analysis procedures so that each constitutes a cluster of characteristics correlating highly with each other, but negligibly with items in other scales. The results they report on more than 1,000 men employed in many different occupations show a number of significant differences between professional and managerial, skilled and semiskilled, and clerical and sales groups. A report by Chaplin (32) available so far only in abstract form shows that the stereotypes of social class groups are related to some scores on the Strong test. The Strong OL (occupational level) scale has been shown by Ostrom (33) to be related to the ratings on amount of drive which are given twelfth grade students by teachers and classmates. In this high school group, however, the correlation with academic grades which a few college studies have shown did not appear,

The most complete study that anyone has made so far of the relationships between Strong scores and other measures of personality has been published by Cottle (34, 35). His subjects were 400 male veterans who were

given the Strong and Kuder interest blanks and the Multiphasic and Bell personality inventories. Each score was correlated with all the others and a factor analysis made of the results. Of the seven factors that could be identified, none was common to both personality and interest tests. These findings substantiate previous indications that the traits we are measuring by means of our interest blanks and personality inventories are largely independent of one another.

Vernon (36) has approached the problem of classifying high-level occupational interests without the use of tests. He chose 58 varied occupations, arranged them in pairs, and asked judges to rate each pair as to how similar the two occupations were. A factor analysis of the resulting inter-columnar correlations produced four factors which he called: (*a*) Gregarious *vs.* Isolated, (*b*) Social Welfare *vs.* Administrative, (*c*) Scientific *vs.* Display, (*d*) Verbal *vs.* Active. It is interesting to note the similarity between these factors and some of those on which the Strong groupings of keys are based.

General Discussions of Mental Organization

Questions as to the relative importance and the interrelations of general mental ability of some sort and narrower abilities of the types disclosed by factor analysis still persist. Results reported by Curtis (37) cast doubt on one commonly-held hypothesis, namely, that abilities become more highly differentiated as mental growth progresses. He used 10 tests made up of numerical, spatial, and verbal materials in two difficulty levels. Both forms were given to a group of nine-year-old boys and a group of twelve-year-old boys. Correlations were analyzed by Holzinger's bi-factor method. The significant findings were that the loadings for the general factor turned out to be no larger for the nine-year-old than for the twelve-year-old group, but that at both age levels the easy form of the test produced a larger general factor than the more difficult form.

Burt (38) has made a comprehensive review of the factor analysis literature and has summarized the results within a hierarchical framework that he has used as a basis for all of his own work. According to this scheme, the lowest level is that of simple sensory processes; here research shows a large number of highly specialized factors for different kinds of content. Proceeding up through subsequent levels, perceptual and motor processes, the intermediate or associative level, and the higher relational level, one finds that the higher one goes in the pyramid, the more highly-organized are the processes involved and the more general are the factors that mathematical analysis suggests. Burt's is a catholic system with room in it for temperamental as well as cognitive factors at each level, for both form and content factors, and for general speed and attention variables that cut across all levels. It is difficult to do it justice in a brief review.

Another representative of the British factor analysts, Thomson (39), makes a searching criticism of some of the assumptions made by both British and American users of these methods. He again states the case for his own theory which postulates a multitude of small overlapping factors as a basis for explaining the intercorrelations of tests.

In the field of personality study, Fiske (40) has made an interesting contribution by comparing factor patterns obtained from three separate sets of ratings on the same group of individuals. The subjects were 128 psychology graduate students in the clinical training program. Ratings for each individual on a number of traits taken with some modification from Cattell's personality study were obtained from three team-mates, three staff members, and the individual himself. There was considerable agreement in factor loadings derived from the three matrices. Rank difference correlations between sets of loadings showed highest agreement for the "Emotional Control" factor, lowest for "Confident Self-Expression." Agreement for "Social Adaptibility," "Conformity," and "Inquiring Intellect" was significant but far from perfect.

Several attempts to think through the problems of personality organization have come out during the year. Thorndike (41) has attempted a synthesis of factor analysis findings with regard to both abilities and personality traits. He, like Burt, thinks in terms of a complex, hierarchical organization but differs from him in the nature of the organization he proposes. Approaching the problem from another direction, Duffy (42) maintains that personality can be described in terms of direction and energy mobilization. "The broad outlines of personality can be usefully sketched in terms of what the individual approaches or withdraws from, with what intensity, and with what consistency." Wolfle (43) does not discuss personality organization in general but suggests that it can be studied for any individual by observing the changes he imposes upon any type of material. This principle can be used as a basis for evaluating tests and for developing new ones.

One of the trends that is becoming apparent in the thinking of various people [see Burt (38)] is the abandonment of the attempt to distinguish in a clearcut way between cognitive and motivational traits as we are able to measure them by our tests. The most definite spokesman for this point of view is Wechsler (44). He proposes a concept of intelligence as a manifestation of the personality as a whole rather than of purely intellectual factors. He brings factor analysis results into juxtaposition with clinical practice to show that noncognitive factors are ever-present in cognitive tests.

A number of recent research studies give some support to the view that personality factors have some effect in determining performance on all kinds of tests. Staudt (45) shows that error scores are more closely related to maladjustment as measured by the Minnesota Multiphasic than are scores on items correct. All correlations are rather low. Cronbach (46) has been accumulating a considerable amount of evidence on the effects of what he calls response sets in diverse types of test. Examples of these sets are tendency to guess, working for speed *vs.* working for accuracy, etc. Subjects show some consistency in these sets where test situations are similar, and their preferences show at least a slight relationship to some of their Bernreuter and occupational interest scores. Rundquist (47) reports that the tendency to take an extreme position when asked to register an opinion on a five-point scale is also somewhat consistent from one test situation to another. Both Cronbach and Rundquist feel that because of our ignorance as to what such re-

sponse sets are showing about personality, it would be better to design tests in which they do not appear than to try to use them for diagnostic purposes. Multiple-choice items in tests of a difficulty level suitable for the persons taking them seem to be most free from these complications.

Attempts to assess the influence upon test or real life performances of those personality factors which we have devised ways of measuring still leave us grappling with considerable uncertainty. Cronbach (48) repeated with University of Chicago students the kind of Rorschach study that Munroe (91) had previously carried out at Sarah Lawrence. In contrast with her results, he found that Rorschach scores added nothing to scholarship prediction and showed only a low correlation (about .3) with adjustment ratings. The fact that we do not know the nature of the difference in the two college populations which produces these discrepant results makes for much doubt as to what the Rorschach method shows about personality within the normal range. The report by Wells (49) on 10 highly intelligent students who had adjustment problems, a part of the Grant study, leaves us with the feeling that none of the techniques used, Sheldon somatotyping, Rorschach testing, or handwriting analysis, seem to describe these people very well or indicate the nature of their adjustment difficulties.

Negative results from an entirely different sort of attempt at personality measurement are shown in a study by Kleemeier & Dudek (50). They tried to find evidence for a general personality trait of flexibility-rigidity by including in a battery of tests to be factor-analyzed several tests requiring shifts from one task to another. They came out with four factors obviously based on the content or type of materials used in the original tests. There was no factor for which loadings could be interpreted in such a way as to differentiate between "shift" and "nonshift" tests.

A coherent, reasonable theoretical structure into which individual differences in personality can be fitted is still lacking. We may agree with Wechsler that cognitive and noncognitive elements are inextricably interwoven in the individual's performance on all types of tests, but much more work will need to be done before we can draw any definite conclusions as to what the basic variables are.

Group Differences

Sex differences maintain their perennial interest for writers and research workers. Mead's book, *Male and Female* (51), is the major publication for the year on the topic. It consists of a revaluation of the data from primitive societies previously reported, an arm-chair analysis of the situation in our own culture, and some generalizations as to what sex roles should be. Mead steers clear of both the extreme equalitarianism emphasized by the feminists and the extreme stress laid on female passivity and dependence by some psychoanalysts. The principal conclusions are: (*a*) a person is a human being first, a member of one sex second; sex stereotypes misclassify and limit many individuals in all cultures; (*b*) there can be no progress in the well-being of one sex at the expense of the other; they must flourish together if society is to be

healthy; (*c*) all kinds of creative activity—art, religion, etc.—need both male and female contributions.

One set of findings that seems difficult to relate to anything else is presented by Warner (52) as a byproduct of his investigation of the color preferences of psychiatric groups. While few trends related to psychiatric diagnosis were apparent, there were consistent sex differences in all groups. Males were more likely than females to prefer cool to warm hues and extreme to medial colors along both the lightness and the saturation dimensions. Whether these same differences would characterize normal groups and whether they correlate with differences of some sort in emotional response are questions still unanswered.

The emotional and motivational differences between the sexes which psychoanalytic writers have been postulating for some time have recently come in for some experimental study. Franck & Rosen (53) worked out a projective test of masculinity-femininity which required that subjects make drawings from incomplete figures. Male and female college students showed significant differences in their characteristic ways of accomplishing this task. The authors think that the results can be explained in terms of differences in "body image," which term is defined broadly enough to include both structure and impulses. Thus, an individual's performance may show indirectly the extent to which he accepts his sex role.

Psychoanalytic formulations of sex differences receive even more striking corroboration in the work of Blum (54). He developed a cartoon type of projective test in which the pictures represented such aspects of psychosexual development as oral eroticism, oral sadism, Oedipal intensity, and castration anxiety. The subjects, again college students, were asked to write stories about the pictures. These were scored or rated on the degree to which each of the psychosexual variables seemed to be present. He was concerned with two questions: (*a*) whether the correlations between variables would agree with the relationships discussed in psychoanalytic texts, and (*b*) whether the sex differences would agree with those discussed in the texts. To both questions, the answer was definitely positive. In the part of the study concerned with sex differences, eight of the nine variables on which one would expect sex differences on the basis of statements made by Freud or Fenichel actually showed statistically significant differences in test score.

On the topic of motivational differences, one other brief paper by Sumner & Johnson (55) is deserving of mention. Their subjects, 52 Negro college students, showed significant sex differences in level of aspiration, with women consistently lower than men.

Few publications on race and nationality differences have appeared during the year. Pastore (56) raises again the old question as to the validity of the Negro-white comparisons based on World War I army data. He argues that the large percentage of zero scores among the Negroes proves that the test was not a satisfactory measure of their ability. Strong (57), in a paper so far available only in abstract form, examines some data from a doctoral thesis by Hartshorn which showed that the interests of Negro lawyers, life

insurance salesmen, and physicians differ from those of corresponding white groups and discusses various possible explanations.

Difficulties encountered in attempting to use psychological tests in different cultures have been discussed in two reports. Notcutt (58) shows how the Zulu distribution on the Progressive Matrices Test differs in skewness from the English distribution. Scott (59), who developed a test for measuring Sudanese intelligence, explains how important it is to prepare the right emotional atmosphere and allow more explanation, more practice, and more time. He shows also how some familiar types of test item are completely inapplicable in this culture.

An anthropological study of 50 subjects in the Isle Vert, using Rorschach evidence to analyze the type of personality common to the group, has been made by Mathieu-Fortin (60). The group personality is described in some detail, but the full data from the Rorschach protocols themselves are not given in this publication.

A novel approach to the problem of differences in national character is represented in a report by Cattell (61). He has made a factor analysis of statistical data for the century from 1837 to 1937 on 69 national groups. Ten factors emerged, eight of them with plausible psychological or sociological interpretations. These eight are: (*a*) size, (*b*) cultural pressure *vs.* direct ergic expression, (*c*) enlightened affluence, (*d*) thoughtful industriousness *vs.* emotionality, (*e*) vigorous, self-willed order *vs.* unadapted perseveration, (*f*) bourgeois philistinism, (*g*) Buddhism-Mongolism, (*h*) cultural integration and morale. More research along these lines is planned. Its object is to identify the basic dimensions along which differences between national groups lie. The principal question with regard to this method of research would seem to be the relationship of the census data on income, education, etc., to real psychological characteristics of the people. It is more apparent for some types of things than for others.

That social class position is related to measured intelligence is once more indicated in a paper by Blake (62). Significant positive correlations between intelligence test scores and Sims Scorecard ratings were found for students in advanced college classes. Selective factors were not discussed, but since they ordinarily operate to bring to college a much smaller proportion of lower-level than of upper-level students, it would seem that these results are at least as significant as they appear to be.

Milner's study (63) was concerned with the effects of both sex role and social status on early adolescent personality. The subjects were 30 boys and girls from the Midwest study for whom unusually complete personality information was available. An elaborate statistical analysis of differences in a large number of tabulated traits resulted in a description of characteristics typical of the "Common Man" class (upper-lower and lower-middle) and of boys and girls within this class level. Group-typical characteristics are high anxiety level, immature, dependent inner life making ties with peers difficult, and need for approval which can be obtained only through conformity. Characteristics typical of boys are concern over impulse-control

resulting in difficulties with teachers and underachievement, and ability to form close emotional ties with selected people. Characteristics typical of girls are suppression of overt impulse expression, a drive for conformity which keeps up school achievement but retards creativity and imagination, and the absence of close emotional ties. One could wish that such generalizations could have been based on more than 30 cases, but the study constitutes at least a starting point for further research.

Our understanding of extreme deviates at the low end of the intelligence scale has been facilitated by the publication of new texts on mental deficiency by Wallin (64) and Sarason (65). Wallin's book is the broader of the two in its scope, including discussions of general issues such as classification and causation as well as summaries of the work on the various clinical types. Sarason places particular emphasis on what he calls the "garden-variety" of deficiency, which holds the greatest interest for most psychologists. His approach to problems of feeble-mindedness fits in with the trend noted in the previous section toward dispensing with the distinction between intelligence and personality. His discussion of psychotherapy is particularly interesting since, heretofore, the mentally deficient have usually not been considered suitable subjects for such treatment.

Study of the extremely gifted is represented by a new book by Tsanoff (66), a philosopher who relies mainly on sources such as notebooks, journals, and biographical writings as a basis for conclusions. However, his ideas are consistent with those of the leading psychologists who have worked in this area in that he emphasizes the importance of "creative intelligence" in all fields of achievement and assumes that this ability differs in degree rather than in kind from that shown by the average man. Lehman (67) has published another in his series of studies, this one showing by quotations from biographies and other official sources that a considerable number of first rate contributions in scientific, literary, and artistic fields were made by persons under 21 years of age

From the specifically psychological viewpoint, the most important work is Roe's study (68) of a group of 20 distinguished biologists in which she has made use of several types of test as well as interview materials. High intellectual ability characterizes all these men, but there is some differentiation between verbal and mathematical abilities that seems to be related to field of specialization. Personality traits indicated by the Rorschach and Thematic Apperception Test (TAT) include lack of aggressiveness, lack of interest in interpersonal relations, restricted psychosexual development, conventionality, and sense of responsibility. A group Rorschach study of 188 biologists (69) showed somewhat similar trends.

Factors Related to Individual Differences

The major contribution of the year in the field of physical-psychological relationships is Sheldon's book (70) on delinquent youth. To his previous classifications of physiques and temperaments, the author has now added a psychiatric classification in which the extreme of each component represents

a pathological deficiency in one of the three temperamental factors. Most of the book consists of standardized biographies of the 200 young male subjects in which constitutional and behavioral data are combined. Statistics for the purpose of evaluating how well the system of personality description works in these cases are only incidental to the main development, but there are graphs and correlations in the latter part of the book that support the constitutional interpretation.

Hunt (71) has written on the somatotype problem, proposing that the three primary components of physique be understood as the predominance of the characteristics related to three different growth stages rather than in terms of the embryonic layers first suggested by Sheldon. According to Hunt's view, endomorphy represents the growth trends reaching their peak at nine months, ectomorphy those reaching their peak at nine years, and mesomorphy those reaching their peak at adolescence.

Investigators seeking evidence as to the relationships between specific physical and psychological characteristics have met with the usual failure to obtain positive results. Smith (72) found neither activity nor its absence to be consistently related to the menstrual cycle in groups of industrial workers. Lacey, Siegel & Siegel (73) did not get significant correlations between morphological index and galvanic skin response for their college subjects. Kreezer & Smith (74), correlating electroencephalographic measures and intelligence ratings of nondifferentiated familial mental defectives, found only insignificant relationships.

Brozek (75) has summarized the results of the starvation research project, most of which have been previously reported. The changes in Minnesota Multiphasic Personality Inventory (MMPI) scores during starvation and their gradual return to normal during rehabilitation are the most striking findings of a quantitative nature.

On the nature-nurture controversy, a new approach is presented in a book by Pastore (76). His thesis is that hereditarian and environmentalist views are related to conservative and liberal attitudes on social, political, and economic issues. From the writings of prominent psychologists, sociologists, and educators who have taken a strong stand on the nature-nurture issue, he cites evidence that such a relationship exists. Only 2 of the 24 persons discussed seem to be exceptions to the general trend.

Not much has been said on the hereditarian side of the argument during the year. Two new twin studies have appeared. Anastasi (77) reviews a Swedish work by Smith in which differences between identical and fraternal twins in imagery type and qualitatively-judged personality traits are compared. As usual, the identicals are more similar than the fraternals, but Anastasi feels that the study is open to criticism on the grounds of small numbers and subjectivity in judging. Kallman (78) presents a first report from a continuing study of 1,602 senescent twin index cases. There is evidence that the monozygotic, even at this advanced age, are more alike than the dizygotic in appearance and physiological characteristics, and that their life spans are more likely to be similar. On several kinds of tests given to 77 twin pairs,

46 of which were identical, differences in score were significantly smaller for the monozygotic than for the others.

Several studies have emphasized the influence of environmental factors on measured intelligence. Skodak & Skeels (79) have made their final follow-up study of the adopted children first tested before 1936 in connection with placement procedures. The above-average IQ's reported in previous follow-up studies have been maintained. The average IQ for the children is 20 points higher than the average for their real mothers. Correlations of children's with true-parents' IQ have increased, however, from each follow-up to the next, and the most recent scores do not correlate with foster-parent education. For the children, there are substantial positive correlations between successive test scores. Apparently a favorable environment has raised the level of the whole group, but has not erased the differences between them. Axline (80) reports the cases of five apparently feeble-minded children who gained 20 or more points through play therapy. She is unable to explain why other cases, which she also reports, did not gain.

Davenport & Remmers (81) have studied the characteristics of states that are related to average scores made by men from those states on the Navy A-12 and V-12 tests. A factor analysis of these characteristics shows that income, urbanization, and geographical location (South *vs.* non-South) are the important differentiators. Altus (82) presents some unexplainable correlations indicating that within a group of Army illiterates, intelligence is positively related to years of schooling when degree of literacy is held constant. One would not expect that the length of time a person managed to remain in school without learning to read would be an indication of intelligence. The correlations Altus gives are too low for statistical significance but are consistent in their direction.

Certain specific questions related to the heredity-environment issue have been settled rather conclusively during the year. Bayley (83) has shown from the records of 40 persons tested at frequent intervals from birth to 18 years that in the majority of cases the IQ is not constant over a long period of time. While the correlations between adjacent tests from the age of six on are .7 or higher, there are great individual differences in growth curves plotted from either IQ or sigma scores. Lability is not related to sex or to IQ. The data for the group show considerable fluctuation in both means and standard deviations from age to age. There is a consistent tendency for variability to be low at some ages, high at others. The author thinks that this trend cannot be explained as an artifact in test construction but means that the homogeneity of a group is greatest at the time a certain kind of ability is just beginning to develop, least when it is in process of development, and greater again after it has matured. Besides their practical value in promoting caution in the use of intelligence test scores, especially at early ages, for predicting later intellectual levels, these results constitute important basic material for theories of mental organization.

The other widely-discussed question to which a decisive answer has recently been given is whether or not there is a constant decline in intellectual

level from generation to generation because of differential birth rates. For one country the answer is "No" so far as tested intelligence is concerned. Results from an important research project carried out in Scotland have been discussed by Maxwell (84), Burt (85), and Vernon (86). In 1947, the Scottish Council on Education repeated a survey of the entire school population within a certain age range that they had made in 1932. While the customary negative correlation between IQ and family size appeared here as elsewhere, comparison of averages showed that over the 15-year period, there had actually been a slight increase rather than a decrease in score. One can postulate such factors as better health, increasing familiarity with tests, migration trends, etc., but it hardly seems likely that they would have been of enough importance to reverse a trend making for a real decline.

Perhaps we are reaching the place where we can turn our backs on profitless arguments and work out a theory of intelligence that will fit all the known facts. This year has produced two such attempts. Knehr (87) wants to define intelligence in terms of flexibility in response possibilities based on the degree of differentiation in the organism, particularly the elaboration of its central nervous system. According to this view, we could never measure intelligence directly but could infer it from test performances. In children, test behavior could actually show flexibility and multiplicity of response possibilities; in adults the behavior we observe would represent the differentiation that had come as an effect of the flexibility that had existed at an earlier time. Thus, an extensive and finely-differentiated vocabulary in an adult would show us that he must have had a finely-differentiated central nervous system in order to acquire it. An even clearer formulation has been made by Hebb [(88) pp. 275–303] in connection with his general theory of behavior organization. He postulates two kinds of intelligence: (*a*) the innate potential determined by the elaboration of brain structure and (*b*) the average level of performance or comprehension, which has been determined to an unknown extent by early learning, a concept upon which Hebb lays great stress. It is, of course, only (*b*) that our tests are capable of evaluating. Most of the heredity-environment controversy evaporates when we realize this fact. Though Hebb does not discuss the point, this theory accommodates very satisfactorily the findings taken up in an earlier section of this review leading to the conclusion that tested intelligence is never free from temperamental or motivational components.

Main Trends

Viewing this large quantity of published material which represents the solid precipitate from a much larger amount of research and thinking, it is difficult to summarize the principal contributions which the total makes to our whole body of knowledge in this subject-matter area. One can, however, include most of what has been done under three headings and point out what is most significant under each.

New leads for the identification of important individual characteristics.— The ethnocentrism research and the related work with the more subtle F

scale have identified a new trait and opened up many research possibilities. The work on perception of the upright and other related perceptual characteristics has pointed to a kind of measurement of individuals that may be of basic importance when we know more about what the differences mean.

Controversial issues upon which some decisive evidence has been obtained.—We have at last something more than clinical observation and common beliefs to substantiate the idea that there are basic emotional differences between the sexes. (We need, of course, evidence from populations other than college students before we can be very sure of what the differences are.) The problem of IQ constancy has been taken out of the realm of argument now that we have the records of a group studied from infancy to maturity. There is clearcut evidence that in one country, thoroughly studied, measured intelligence is not decreasing.

Unsolved problems and uncompleted tasks.—There is still no completely satisfactory model for the description of mental organization, but factor analysis studies on specific problems have contributed more of the basic material from which such a model will eventually be built. It is possible that by abandoning the attempt to separate cognitive from motivational traits, we will hit upon some new way of organizing the facts that we now have. By centering our attention on the characteristics which human individuals consistently show regardless of whether they are unlearned or learned, we may make more progress than we could if we attempted to maintain a strictly hereditarian or environmentalistic orientation.

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PERSONALITY[1]

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Throughout its long history, by asking delimited questions which might be answered by the employment of its unique method, science has moved from its early embeddedness in philosophy to an increasing differentiation from it. Thus it has been that the many separate and distinct sciences have come into being. In recent years, however, there has been a growing recognition that the rigid compartmentalization of science into separate sciences has often served as a barrier to the fullest unification of scientific knowledge. Not only within specific sciences has the need been felt to break down the artificial barriers between subregions of investigation in order to correlate more effectively all knowledge of the field, but between sciences, too, the need for coordination of fact and theory and method has been increasingly recognized. These two trends—the one toward greater integration within the separate sciences, the other toward greater coordination among all sciences—have been clearly evidenced in recent years, not only in psychology, but also in that differentiated region of psychology which has concerned itself with problems of personality.

At the intrascientific level, there is increasing awareness within psychology that even the "simple" processes cannot be either fully described or validly explained if considered in isolation from other processes. What a person perceives, for example, depends upon his needs; what he wants depends upon what he has learned. The necessity of recasting the problems of psychology on a level of adequate complexity and with full recognition of functional interrelationships is repeatedly stressed, and for an increasing number of psychologists the concept of personality—personality as macrocosm—is seen as providing the needed frame of reference for all psychological data.

At the interscientific level there is at the same time a clear realization of the microcosmic role of personality when viewed in the context of its enveloping society and culture.

The attempts to discover the determinants and functional relationships of personality, on the one hand in other psychological processes, and on the other hand in societal and cultural factors, may be seen as most characteristic of theory and research in personality during 1949 to 50. In addition to numerous single articles, a many-authored, major publication devoted to each of these areas appeared during the year: "Interrelationships between Per-

[1] This review covers the period from approximately June, 1949 to June, 1950. The author wishes to express his gratitude to William F. Dukes for invaluable assistance in the preparation of this review.

ception and Personality: A Symposium" in the September and December issues of the *Journal of Personality* (1), and the Proceedings of an Interdisciplinary Congress on *Culture and Personality* (2).

For convenience in ordering the year's literature on personality, the articles and books reviewed in the present chapter will be grouped according to their major emphases or contributions in respect to four questions: (*a*) What is personality? (*b*) What methodological problems are encountered in the study of personality? (*c*) What are the determinants and processes of development of personality both within and outside of the person? (*d*) What are the dynamics—action and interaction—of personality? It is, of course, recognized that a work which has addressed itself primarily to one of these questions may also have made contributions to the other areas of inquiry, and where such has been significantly the case, the work in question will be cited more than once.

What is Personality?

In his attempts to define personality, the psychologist has sought to describe its phenomenal appearance, to discover its substantive nature, or to conceptualize its theoretical structure.

In current attempts to define personality, psychologists have been primarily concerned with two problems: (*a*) the clarification of the concept of personality in relation to other kindred concepts such as individual, person, self, character, temperament, etc.; (*b*) the discovery of those units out of which the personality may be conceived to be built and the determination of the locus of these units upon the dimension of specificity-generality.

Personality and kindred concepts.—One still finds discussions introduced by such phrases as, "By personality, I mean—"; one still finds semantics made the scapegoat for contradictory conclusions. But a concern with definitions as such has almost disappeared from the literature on personality.

Bidney (3) seeks to develop a psychocultural definition of personality and to distinguish it from that of person. Hilgard (4) discusses the concept of self in relation to human motives. Coutu (5) in his book, *Emergent Human Nature*, emphasizes the relativity and probability aspects of personality. Personality is described as a system of tendency—tendencies in situations—or, in other words, a system of probable behaviors under stated conditions.

The argument for specificity.—Thorndike (6), in discussing the problem of personality structure, argues that the organization of personality has no true analogue. The closest one can come to it, he suggests, is to compare the person to a mail-order house into which a multiplicity of orders come from various parts of the country, are routed to their appropriate departments, filled, and dispatched to their points of origin. Thorndike sees the attempts to discover evidence of general factors in personality as not fulfilled; he interprets the results of recent factorial analyses of character and temperament as forcing the acceptance of a large measure of specificity in human behavior. As for traits, Thorndike believes that "when the investigation goes far enough or deep enough it is likely to reveal that the trait is not unitary,

but a collection of more or less independent features of a person" (6, p. 143). In coming to this conclusion, Thorndike focused his attention upon one type of data to the exclusion of contradictory findings which have been reported in the literature. In so doing he has again thrown his support in favor of situationism and specificity, despite the fact that the controversy of specificity vs. generality in personality structure has been shown to require a more complex and subtle resolution than that which is offered by Thorndike's article.[2]

Factors.—In his factorial studies of personality, Cattell (7), using the concept of "personality sphere," claims to obtain a truly representative choice of variables which are relevant to the totality of behavior that is important to man. In a highly ambitious program of research, he sets as his task the discovery of how the primary factors of personality (isolated in his earlier studies) are modified in their patterns with age, sex, cultural status, etc. Paradoxically, the very strong points of studies of this sort are at the same time the sources of their weaknesses. Cattell seeks to investigate personality where personality is most naturally exhibited, in the world of daily living, and in doing so seeks the currently emphasized objectives of personality research: objectivity and complexity. He strives for objectivity by using a rating scale in the use of which raters have been well indoctrinated; but since the ratings are made by close associates of the subjects, distortions and projections are not ruled out, even if objectivity is defined in terms of interrater agreement. He strives for complexity by having a large number of traits rated; yet the very rationale of factor analysis is based upon the assumption that phenotypical complexity can be reduced to genotypical simple structure.

The often expressed criticism of factor analysis, that as the method of analysis varies the factors or simple structure which are revealed will also vary, may be voiced again, for Banks (8), using Cattell's (7) data on women but employing Burt's summational method of factor analysis, extracts a set of factors different from those reported by Cattell. Being able to describe the data most adequately with four factors instead of the twelve listed by Cattell, Banks concludes that "the remaining eight bipolar factors obtained by Cattell are either artifacts or else devoid of statistical significance" (8, p. 218).

A comparison of two sets of personality factors, the one yielded by Cattell's studies, the other by his own investigation, both employing factor analysis, leaves Fiske (9, p. 341) "with feelings of both optimism and discouragement" concerning the possibility of eventual agreement upon the basic variables of personality. Employing the data collected in the 1947 summer assessment program of the Research Project on the Selection of Clinical Psychologists, Fiske sought to determine the consistency of the factorial structures of personality ratings from different sources. The sources were (*a*) pooled ratings of a staff team of three experienced psychologists, (*b*) the

[2] See the discussion of the *P* technique, p. 116.

median of ratings made by three teammates of the subjects, and (*c*) the subjects' ratings of themselves. The traits rated by each set of raters were 22 adopted from Cattell's list of 35.

In each of the three separate factor analyses of staff-, teammate-, and self-ratings, five factors were located. One factor found in each analysis, and essentially a general factor, was labeled Social Adaptability. Three other factors found in each analysis though showing varying degrees of congruence or difference of pattern from study to study were labeled Emotional Control, Conformity, and Inquiring Intellect. A fifth factor of Confident Self-Expression was discovered in ratings by teammates and in self-ratings, while a factor, not identifiable, was found in staff-ratings.

In respect of the comparison of factors isolated in the Cattell and Fiske studies, it is significant that the set of factors yielded by the analysis of teammates' ratings in Fiske's study resembled more closely those found in Cattell's study (which also used peer ratings) than did either of the other two sets of factors. Yet in this case of peer ratings, and even more so in the case of staff- and self-ratings, many discrepancies were found, leading Fiske to conclude (9, p. 341),

> Even in the face of no complete congruence, the similarities support a belief in the possibility of eventual agreement upon the basic variables in personality. Yet, with such comparable rating scales and subjects, why are the results not more similar? Did differences in factorial techniques affect the findings?

A recurrent objection to attempts to reconstruct personality from factors yielded by factorial studies is that they tell us nothing about individual personality structure, since they are based on differences in the behavior of many individuals placed in similar situations. Cattell & Luborsky (10) believe they have at last answered this objection by their development of a system of factor analysis which they claim is capable of revealing the unique trait structure of the individual. Their method, which they call the *P* technique, takes account of the differences and similarities in the behavior of a single individual as he is placed or places himself in many different situations. Such a factorial method may well prove to be of the greatest clinical and experimental value. It may, for example, help to throw light on the perennial problem of the generality of traits. It is conceivable that by treating with the *P* technique the behaviors of a single individual in a representative sampling of situations, it may eventually be possible to put to a crucial test the validity of the claims for specificity of the Hartshorne and May studies. In its present stage of development, however, the unwieldiness of the *P* technique militates against its extensive use.

Types.—The lively interest of earlier years in traditional typologies has all but disappeared from the psychological literature. Albino (11) reports some evidence in support of Luria's stable-labile dichotomy as applicable to normal as well as to clinically abnormal individuals. Gray (12) describes procedures for the treatment of ambiguous scores earned on the Gray-Wheelwright test for Jungian types.

The fact that current literature reveals little interest in the traditional typologies does not, however, mean that typologies as such have been discarded by psychologists. Rather the argument could be made that typologies in 1950 appear in a much more vigorous and infinitely more sophisticated form than they did in yesteryear. Whereas then they were attacked on statistical grounds, today statisticians offer us procedures which, it is claimed, will lend objectivity to the search for types of personality. Such a method is the inverted factor or *Q* technique, developed by Stephenson (13), which yields types of personality by correlating persons on tests rather than, as in traditional factorial methods, correlating tests on persons.

The final report on *The Authoritarian Personality* by Adorno, Frenkel-Brunswik, Levinson & Sanford (14) is a handsome contribution to typology, which, while focusing attention upon the correlates of prejudice in the structure of personality in its underlying dynamics, as well as in manifest behavior, sheds light also on the correlates of freedom from prejudice. These investigators prefer the term syndrome to type, and if by this they mean to underscore the differences between their approach to personality and the traditional typological approach they are certainly justified. For, whereas the method of Theophrastus was deductive—selecting a dominant trait of personality and deducing specific modes of behavior from it—and that of Spranger intuitive, the current work is empirically grounded, and not only in overt behavior but in subjective and implicit behavior as well.

The theoretical formulation of the central core of the authoritarian personality is the result of several independent approaches. It is one which offers striking evidence of levels of personality and something of the conditions of communication between these levels. If there are those who still believe in the possibility of a simple inference of a one-to-one relationship between superficial behavior and motives, fantasy, etc., *The Authoritarian Personality* offers a striking refutation of their position.

The investigators believe (14, p. 971) the most significant result of their intensive study of authoritarianism to be the

> demonstration of close correspondence in the type of approach and outlook a subject is likely to have in a variety of areas, ranging from the most intimate features of family and sex adjustment through relationships to other people in general, to religion and to social and political philosophy.

Early parent-child relationships set a pattern of behavior which, like a mold, leaves its imprint on all later behavior.

Though not intending to dichotomize humanity, this research isolates two types of outlook, namely, the authoritarian, exploitive, and the affectionate, equalitarian, permissive. The latter is, of course, the more heterogeneous, but the former also presents sub-types, e.g., the conventional and the psychopathic.

The social implications of the major findings of this study are indicated. The authors do not belittle any constructive attempt to stamp out the disease of prejudice, but warn that symptom-treating is not a cure. The cure will

involve, in addition to a constructive handling of the many economic, social, political, etc. determinants of the disease, a positive and long-term treatment of the dynamic factors of personality organization.

Methodological Problems

However much psychologists may disagree about the best way to conceptualize the structure of personality or to define its units, on one point—its complexity—there is unanimity. There has, accordingly, been no dearth of attempts to develop tests for the measurement of the many facets and dimensions of personality or of attempts to develop statistical procedures for the quantitative manipulation of personality data. Other articles in this volume review the year's contribution to these areas of investigation. The present section is limited to a brief survey of current attempts to formulate more basic methodological principles of research in personality.

A fundamental principle of personality research, namely, that "an individual reveals his own personality through any change he makes upon any type of material," is briefly presented by Wolfle (15). The rich implications of this principle for psychology, which is becoming increasingly effect-oriented, are not, however, developed or made explicit by the author.

To those who observe and stress the more subtle differences in personality structure as conditioned by cultural products and institutions, Klineberg (16) makes a plea for the use of more adequate sampling techniques and more rigorous quantitative procedures in the collection and treatment of data. Concerning the all too frequent, too glib discussions of traits of "national character" he asks (p. 135), "how basic? how similar? how universal are these traits?" Despite the objections of some anthropologists that many of their data are not amenable to statistical treatment, Klineberg's questions must eventually be answered. Certainly there should be no objection to treating "tender-minded" material in a "tough-minded" way. In an area as complex and confusing as that of personality and culture, every precaution must be taken against overgeneralization and glib interpretation.

London (17) questions the extent to which the principle of determinism can be applied universally and without exception to the development and functioning of personality. Borrowing the concepts of convergence and divergence from the physicist, Langmuir, he argues that many indeterminate processes may, through convergence, result in definite and specifiable states, permitting the formulation of laws. But he also expresses the belief that an opposite state of affairs sometimes exists, namely the impossibility of prognostication. "Divergence," he argues, "excludes the possibility of total or, in some cases, even significant reduction of individual behavior to uniform generalizations and makes it . . . pointless to ask precisely why an individual has traveled the path he has . . . " (17, p. 184). The fruitfulness of such a position for future research in personality is not clear. If it be interpreted as a proposed alternative to the extremes either of nomotheticism or of idiography to the exclusion of the other position, similar to the probabilistic proposals of Reichenbach in physics and of Brunswik in psychology, few

may wish to quarrel with it. If all it intends is a rejection of the idea that man can objectively study reality without tampering with it and a warning against an extreme reduction to primitive components, then London's postulate may serve to emphasize the need for greater concentration upon the molar aspects of behavior and the field-theoretical description of situations chosen for study in personality research. If, on the other hand, a defeatist attitude—even if in only certain restricted areas—is implied in London's position, its effect upon research in personality could only be impoverishing.

The Development of Personality

The influence of Aristotelian modes of thought upon the psychologist's thinking has been nowhere greater than in his attempts to deal with problems of growth and development. These modes of thought have led him repeatedly to conceive of development as a two-stage process in which the contribution of the organism is seen as pitted against that of the environment. He has asked, "Is this trait inherited or acquired? How much of this behavior is due to heredity, how much to environment? What is man's original nature?" With the increasing acceptance of field theory, these questions are today less often asked in such sharp form. Yet, however much the complexity and interrelatedness of growth factors are recognized, highlighting of either hereditary or environmental factors continues in research and theory.

Man's original nature.—The question of the organism's contribution to personality (as distinguished from the environment's), when pushed to its logical extreme becomes a discussion of man's original nature or of his pristine personality. Those who have speculated on this problem have usually joined one of two camps: those viewing infants as little savages; or those believing that the natural state is the ideal one and that evils are all imposed from without. Under the influence of the doctrine of original sin and Freud's concept of the id, the former view has in recent years enjoyed wide favor. Now, however, Maslow (18) rises up to defend the other view; he proclaims that man's animal nature has been much maligned. Arguing that culture is not necessarily instinct-controlling but instinct-gratifying, he attacks the assumption that instincts are invariably cruel or monstrous. After citing incidents to show that the primitive is not necessarily bad, he points out that needs for safety, love, and beauty are just as instinctoid as drives toward war and crime. He claims that current evidence more and more supports the unlearned character of constructive needs and the acquired nature of the destructive forces in personality. Maslow's views are richly provocative, but they, like those which they oppose, do not appear to be susceptible of crucial test.

Culture and personality.—The interrelatedness and reciprocity of personality and culture have long been recognized. Murphy (19) believes that the relationship is an even more intimate one. His is a double aspect theory; personality and culture are two aspects of a single phenomenon.

Hallowell (20) pushes the problem of the relation between personality and culture back one step further by raising the neglected question as to how

both came into being. The continuity of behavior in the animal kingdom has long been accepted, despite the fact, and the problem which it raises, that man is the only animal who possesses both personality and culture in the sense in which these words are usually employed. It is, Hallowell suggests, not learning so much as the capacity for symbolization in learning which plays the unique and crucial role in the evolution both of culture and of individual personality. Human learning, in contrast to that of other animals, is cultural, and culture is neither built up nor transmitted through simple conditioning, but rather by the complex processes of symbolization, repression, identification, etc. Two features make man uniquely different from all other animals: first, the emergence of a dominant integrative center of the personality and the development of ego-centered processes which Hallowell calls self-objectification (e.g., the fact that "I" can think of "me"); and second, the development of a superego which is capable of unconscious evaluation of ego processes and which therefore serves as a brake upon ego functions.

The inextricability of personality from society and culture is stressed not only by anthropologists but by almost all who study personality. Menninger (21) describes as the two most important emphases in psychiatric thinking today the axioms that (*a*) the personality of the child is intimately connected with society and (*b*) the structures of society differ over the face of the earth. In discussing the aspects of interpersonal relationships most significant for the development of personality, Menninger likens personality development to the process of photosynthesis, society being the medium which supports growth, considerate feelings being the nourishment which is synthesized and given back in the form of a useful life. In this view, the ideational content which is communicated is far less important for personality development than the feeling tone which is expressed in interpersonal relationships. A similar point of view is taken by Joel & Shapiro (22) in their discussion of group dynamics: Feeling is described as the genotype of personal interaction while the content of the discussion is said to be the phenotype. In this conceptualization, three basic reactions constitute the system of genotypes: warmth, hostility, and flight from either of these.

Without denying the importance of emotional factors in the development of the individual in society, de Laguna (23) nevertheless chooses to emphasize the role of rationality both in culture and in the development and organization of personality (p. 381).

> Culture is at once the manifestation and the matrix of rationality. All culture is "ideal," not in the sense that it exists as ideas in men's minds, but because it consists in a set of reciprocal dynamic potentialities in men and things, which are in process of actualization through human action.

Rationality is seen as the most important function of personality—only the rational being can become a person, and only the whole person can exercise his powers of reason. De Laguna arrives at this conclusion because of her belief that it is primarily through the development of native capacity—and

most importantly, the capacity to act and think rationally—that an individual achieves the full complexity of personality. In other words, it is only as a person that an individual can use concepts or engage in any sort of ideal or symbolic form of behavior.

The problem of the more basic, relatively unchanging genotypical structure of personality and its more superficial, varying, phenotypical manifestations, is one not only of personality structure, but also of development and function. According to Linton (24), variations and irregularities within any given culture pattern make chameleons of us all. Contrasted with the individual's "basic personality" are his numerous "status personalities." Generated by society and conceived of as being in large part concessions to it, they come into being because society expects individuals who occupy certain positions in its system of organization to exhibit certain traits of personality, and individuals, in general, meet these expectations. "Such stereotypes may or may not correspond to reality. Once firmly established, they themselves become culture patterns which may be transmitted verbally long after they have lost any justification which they may have originally had" (24 p. 170). The concept of status personality provides a mode of phenotypic description of behavior, and Linton's suggestions as to the origin and mechanism of transmittal of status personalities are valuable. There remain other questions still not adequately answered, e.g., what is there behind the scene which keeps these status personalities organized within themselves and separate from each other and which elicits them as needed? The dynamic relations between basic personality structure and status personalities remain largely unexplored in Linton's work. The present theory appears to conceive of the relationship between basic personality structure and status personalities as like that between body and clothes, a certain type of garment always being worn at a certain type of gathering. But clothes can give at best only superficial information about the body which they cover.

The concept of social role as developed by Newcomb (25) is both like and unlike the concept of status personality. Both refer to and are concepts for the ordering of phenotypic behavior; but whereas the concept of status personality emphasizes changes in patterns of behavior to meet societal expectancies,the concept of social role points more to the persistency and consistency of behavior. Indeed the very fact of the persistence of personality may, Newcomb suggests, be accounted for by the high degree of persistency in role behaviors which are directed to the role responses of others and by the embeddedness of self-perceptions in a given kind of role system. By an impressive marshalling of facts and arguments, Newcomb makes a strong case for the fruitfulness of the concept of role behavior in the study of both personality and social psychology. Role behavior he sees as the unit of study par excellence in both of these fields.

The response of a person to social expectancies, treated by the concepts of status personality and social role, is viewed from yet another angle, that of psychiatry, by Reider (26). His concern is with society's expectation of

normal behavior and the maladjustive response of some to this societal demand. Reider points out that as an individual learns what his society considers normal, he strives to adhere to it in his behavior. He comes to act as he thinks normal people act. But unfortunately for many, the very idea of normality and striving for it becomes a layer of secondary defense, in large part intellectual, which is added to character structure. The result may be an excessive vigilance to conform, which Reider calls "pseudonormality."

Physique and personality.—In this third book on somatotypes and their correlates, *Varieties of Delinquent Youth*, Sheldon (27) presents the thesis that the root and cause of type of delinquency is to be found in the individual somatotype. Sheldon takes pride in stating what he calls the "radical premise" of his position, namely, that behavior is a function of structure. So stated, there is nothing radical about Sheldon's premise; all psychologists make the same hypothesis. None would question that an individual's bodily equipment significantly determines the responses which he will make. But in the psychological climate of today, it is recognized that behavior is also a function of the totality of an inner and outer environment, in short, of a total field. The "radicalness" of Sheldon's position lies in the narrowness of his cognitive map in attacking problems of behavior. In his investigations, "The explanation of a personality . . . is always undertaken against the frame of a taxonomic description of the physical constitution" (27, p. 4). Sheldon conceives of his work as an operational approach to personality which avoids the semantic difficulties which he sees as having plagued the work of Freud. Whereas Freud moved from the conscious to its unconscious substrate, Sheldon asserts that his own method is one by means of which he moves in the opposite direction, from the unconscious to the conscious. The body, he asserts, is the unconscious—the objectification of the most long-standing, deeply established habits, the deepest residual and age-long record of ancestral habits. This position, reminiscent of Jung's collective unconscious and of Lamarck's theory of evolution, will not find a ready acceptance in the scientific climate of today; neither will Sheldon's biased approach to delinquency.

Though one can hardly doubt that Sheldon's technique for the measurement of somatotype is, by all odds, the best yet devised, his system of somatotypy and its presumed relation to temperament have been often questioned. Hunt (28), in contrast to Sheldon, is impressed by the changes in somatotype in the course of a lifetime. He believes that human physiques are more nearly alike at early stages, somatotypes becoming progressively differentiated in later maturation. It is his thesis that endomorphy is the infantile component, reaching a peak nine months after birth. From then until about the ninth year, the body build is primarily ectomorphic. From eight years of age onward, growth is mostly mesomorphic. Associated with these changes in physique, at least in our society, there are, Hunt believes, changes in temperament. Viscerotonia is the infantile temperament with love of sleep, digestion, and dependency. Cerebretonia belongs to early childhood, the period of undisciplined imagination. Somatotonia, Hunt finds difficult to fit

into the scheme, though he points out that the most mesomorphic individuals maintain through life a permanent air of adolescence. The exceptions to the relationships described by Hunt come easily to mind. Despite its long history, the problem of the relationship of body build to temperament remains in need of rigorous investigation.

Learning and personality.—The molding of personality by society and culture is, from the standpoint of psychology, basically a problem of learning. Several of the authors whose work has been reviewed in the section on *Culture and personality* have dealt with various aspects of this problem. Whereas most of them have proposed various complex dynamisms as mediators of the learning process, McGill (29), arguing for a "genetic analysis" as against a "status quo" approach (ahistorical or systematic explanation in Lewin's terms), considers personality formation best explained in terms of association of ideas, classical conditioning, and selective learning. Despite McGill's programmatic position, the fact remains that the complexities of personality development have simply not been most adequately handled by an elementary, associationistic, stimulus-response psychology.

Among more empirical studies of learning and personality formation, Sears & Wise (30) report an investigation of some behavioral aspects of the oral drive and its frustration. Mothers of 80 children were interviewed concerning the age at which their children were weaned, the speed with which they were weaned, and their reactions to weaning. If the oral drive is partly a product of the reinforcement of the sucking act during feeding, the strength of the drive should vary with the amount of reinforcement. To test this hypothesis, the degree of weaning disturbance in the late-weaned group was compared with that in the early and middle groups combined. The expression of rebellion at the time of weaning was significantly greater for the late-weaned, indicating that longer retention of the sucking method of feeding strengthens the oral drive. The data further suggest a greater frequency of thumbsucking among the late-weaned. By dividing the late-weaned group, according to method of weaning, into sudden, gradual, and very gradual, and comparing them on frequency and severity of thumbsucking, Sears & Wise found some indication that it is the strength of the oral drive, and not the amount of frustration of the drive involved in the weaning process, which determines the occurrence of thumbsucking. These data are not interpreted as either confirming or contradicting Levy's findings, since in the present study the strength of the oral drive itself, as well as interference with the drive, is a variable.

Further effects of parental rejection and over-protection on the developing personality of the child are reported by McCarthy (31) in her study of non-readers. Among the non-readers she discovered two quite distinct types of children. On the one hand there were those who were aggressive, quarrelsome, defiant, jealous, and uncooperative, and, on the other hand, those best characterized as submissive, docile, immature, and helpless. The aggressive non-readers presented histories of parental rejection, in contrast to the submissive non-readers whose parents were discovered to be over-

protective and restrictive. The data of this study support the often reported observation that emotional insecurity in children may be the basic cause of educational disability and of failures to learn which are not due to mental defect.

Spache (32), stressing the significance of the concept of self in the development of personality, describes the task of the educator as being that of discovering the child's concept of himself and helping it (the self) to become more consistent and integrated.

Personality and social class.—With data gathered in intensive case studies, Milner (33) has sought to study the effects of sex role and social status on the early adolescent personalities of upper-lower and lower-middle class boys and girls. She finds the major motivation of these children to be the attainment of more love and approval, the greatest problem for the typical boy in these groups being impulse-control, while that of the typical girl, having already learned suppression, is the drive for conformity. Whether the adolescent develops a personality which is typical of his group appears to be less a function of social class per se than of emphases within the home. Here the most crucial factor is the mother because to the extent that her personality approximates the typical feminine personality of her social group, her children will grow up to be typical boys or girls in their group.

In another study of adolescent character and personality, Havighurst & Taba (34) collected a multiplicity of data on a group of 16-year old Middlewesterners: reputation scores with reference to character and social adjustment; measures of intelligence and of personality (using projective tests, tests of moral attitudes, etc.); and information gathered in case studies. Composite character reputation scores were found to correlate relatively highly with such factors as school grades, social class, and IQ. Five empirically derived types of adolescent personality, differentiated in respect of social adjustment, are described in the study: self-directive, adaptive, submissive, unadjusted, defiant, and mixed. Havighurst & Taga's book provides valuable psycho-sociological data on the perception of the mask aspects of adolescent personality in the social milieu of contemporaries; its methodological weakness, however, lies in its lack of clear operational definition of terms.

Centers (35) reports a survey, recently conducted in an Eastern seaboard high school which was designed to sample the social class identifications of American youth. He finds that the class consciousness of adolescents, while closely approximating that of adults, is displaced slightly upwards. This study indicates clearly that group-membership as a determinant of personality has already significantly influenced personality by the time adolescence is reached.

Research on occupation, class, and ethnic status as determinants of personality is summarized and appraised by Komarovsky & Sargent (36). In the first of these categories, the decisive determinants of personality are seen as social interaction on the job, certain technological and economic features of occupation, etc. To combat the errors of faulty generalization

from anecdotal material in this area of study, Komarovsky & Sargent urge more rigorous methods of research. The reciprocal nature of the relationship between occupation and personality also needs more intensive investigation; the selective attractions of different occupations must be considered. That is, sources of strain in a physician's professional life may produce anxiety syndromes, but the possibility that the profession of medicine attracts a somewhat anxious type of person must not be overlooked. The roles of class and color in determining personality have often been reviewed. The authors of the present summary make no new contribution to these areas.

Personality development and psychotherapy.—Sutich (37) would correct the false impression of many that the aim of counseling and therapy is the establishment of an effective but essentially unchanging or static mode of personal adjustment. He stresses the importance for the person of a full awareness of the need and capacity for a continuous emotional development as the basis of the full-valued personality. Development in the sphere of personality is a never-ending process. Neither the end state of therapy nor maturity achieved without benefit of therapy means the banishing of anxiety. Rather, both mean the mastering of anxieties on one level, to become anxious and uncertain on higher levels, with the capacity, however, to achieve creative resolutions of the conflicts experienced on these higher levels of functioning.

The role of self-conception both in the development of personality and as a determinant of attitudes towards others has been stressed in recent years. From Rogers and those who have been influenced by his theories of personality and counseling there come now interesting clinical-experimental findings in this area. Stock (38) shows in correlational studies a clear cut relationship between feelings about the self and feelings about others. Furthermore, as self-feelings change, so do those about others, though their sequential relationship is not identifiable. Sheerer (39) also describes the change in self-evaluation as a result of therapy, and like Stock, shows that the degree of acceptance of and respect for others is significantly related to the attitude toward the self. Since, however, in both of these studies, one of those who categorized and rated attitudes was the person whose hypotheses were being tested and since also all other raters were aware of these hypotheses, the relationships found and the conclusions drawn are less cogent than they would be had ratings been made by independent observers.

Social psychology to Coutu (5) can be adequately set forth only when the principles of personality formation and organization are understood. Personality is seen by him as emerging from units which he calls "tinsits"—that is, tendencies in situations. His book is noteworthy in that it seeks to deal with the full complexity of personality within a formal and rigorously objective frame of reference.

In the light of his review of the experimental and clinical literature on the development of personality, Cattell (40) formulates 17 principles or laws of personality formation. Though one may question whether these are laws

in any true sense of the word, they are, nevertheless, clearly stated hypotheses which will doubtless stimulate numerous researches designed to test their validity.

THE DYNAMICS OF PERSONALITY

Until very recently the stream of psychological research has run in two channels. On the one hand, experimentalists, in imitation of the pattern of research set by physical scientists, have sought to make their investigations as formalistic as possible. Abstracting process from person, they have studied process under rigorous conditions of laboratory control. On the other hand, empirical psychologists have focused their attention on the functionalistic problems of complex modes of behavior and personality, content to study them as best they could under the crude and uncontrolled situations in which they were to be found, e.g., in school, clinic, industry, primitive society, etc. Exceptions, of course, come immediately to mind: Stern's "Keine Gestalt ohne Gestalter," Jaensch's insistence upon perception as the medium for revealing basic personality, etc. Only recently, however, has a real confluence of the two streams of research begun to occur. The rapprochement is urged on two grounds. (*a*) The claim is made (40, p. vii) that

> no law or principle in particular realms of psychology, e.g., perception, reflexology, testing, learning, can be fully understood or applied in prediction without reference to the structure of the total personality. Personality study is the natural hub upon which all more specialized sectors of psychology turn.

(*b*) The view is expressed that knowledge about simple and partial processes of sensation and response will shed light on the person as a whole. From this latter position Klein & Schlesinger (41, p. 39) state that "formal process of a response as well as content . . . refer to styles of adjustment. . . . The psychophysicist may have valuable information to offer us about basic ego processes"; and Werner & Wapner (42) investigate sensory processes as bases for personality differences. The answer to the question as to how personality functions is sought today in all fields of psychology, and every field in turn is seen as illuminated by research in personality.

Personality and perception.—On the basis of extensive functionally oriented investigations of perception, Bruner & Postman (43 p. 16) seek to develop "a unified theory of behavior which treats the organism as an organized whole and which contains laws stating the manner in which perceiving is an instrument of adjustive activity." As part of this program, they and their co-workers have investigated the effect on perception of such factors as value and need, symbolic value, tension and tension release, emotionality, personal value, multiplicity of set, satisfaction and deprivation. Bruner & Postman (43) review this experimental literature on motivated perception and present a full bibliography of it.

In many respects the severest criticisms of this work have come from those who subscribe to the same theoretical position taken by Bruner and Postman. On the basis of a reanalysis and reinterpretation of the original Bruner-Goodman data and a repetition of their study, Carter & Schooler

(44) question the conclusions concerning the effect of value and need on perception. Taking specific experiments as illustrations of each of his points, Pastore (45) argues that the Bruner-Postman and earlier Murphy experiments are inadequately formulated; that they contain frequent statistical errors; and that they suffer from methodological flaws. The theory, he asserts, is not clearly stated; its terms are undefined; and it is generated from ad hoc hypotheses and based upon contradictory results.

Klein & Schlesinger (41) criticize the studies on grounds of their incompleteness. Whereas Bruner & Postman's approach is process-centered, Klein & Schlesinger argue the superior merits of a person-centered approach, warning of "the consequences of ignoring personality structure in perceptual research which ostensibly is personality oriented" (41, p. 32). They interpret the Bruner-Postman studies as demonstrating that value and need can affect perception, but do not always do so. The effect of a drive or of a value-stimulus on perception varies with the particular person and the specific situation; value-symbols may even be fundamentally irrelevant to the task set by the experimenter. Knowing about the process of accentuation is only half the story; we need to know also how much accentuation occurs in each person, and why that much and no more. The insufficiency of generalizations of group tendencies for describing the effects of perception on personality is illustrated by an experiment which they cite on sensory thresholds in the hypnotic state (as yet unpublished). "The generalized hypnotic state itself, inferred from the averaged data, may correspond to no consistent pattern of threshold changes. But the individual subject does show a reliable pattern of change under hypnosis" (41, p. 35).

Different people put perceptual properties (e.g., organizing time and constancy) to work in different ways. These variants, Klein & Schlesinger call modes. These modes, in turn, pattern themselves into syndromes. It is the perceptual syndromes which "reflect for us the integrative principles which give character and distinctness to perceptual life" (41, p. 43). Insofar as organized perceptual systems represent the person, these principles may be used to describe personality. The possibility of eventually building an empirical typology based on these natural clusters is envisaged by the authors. The schema proposed by them for research in perception-personality looks promising, but with Murphy (46) one might wish that Klein & Schlesinger had indicated a predictive test for the relative efficacy of their method as compared with others.

The Klein-Schlesinger framework is a hierarchical one. If one were to search for basic work at the bottom of the hierarchy, he could find no better source than Werner & Wapner's (42) sensory-tonic field theory of perception. From the literature they cite evidence of the effect of sensory stimulation upon the distribution of tonus in an organism. In the relatively simple laboratory set-up of having subjects adjust a luminous rod in a dark room, they demonstrate the formative influence of tonus, either directly induced in the muscles or indirectly created by means of sensory stimulation, in structuring the perceptual field. They find, furthermore, evidence for the

equivalent functioning of the tonic and the sensory: "both affect the organism in an identical way resulting in similar perceptual changes" (42, p. 101). The individual with sensory-tonic energy (attitude, motivation, set) may release it not only in bodily movement but also through perceptual displacement or by projecting it into perceptual objects. Such findings constitute a significant basis for the rationale of projective techniques in personality studies.

Taking what she calls a personality-centered approach, Frenkel-Brunswik (47) looks for forms of perceiving which may hold both in the emotional and social areas and in the cognitive sphere. She finds (p. 140) some evidence of

> a prevalence of premature reduction of ambiguous cognitive patterns to certainty in the prejudiced subjects, as revealed by a clinging to the familiar, or by a superimposition of one or many distorting clichés upon stimuli which are not manageable in a more simple and stereotyped fashion. . . . In the case of distinct intolerance of emotional ambivalence one may as a rule be able to locate at least some aspects of intolerance of emotional ambiguity although these may often be more apparent on a higher level than that of perception proper.

But inconsistencies were about as striking as generalities. Analysis of individual cases led to the conceptualization of the "closeness of opposites" (Jung's enantiodromia). The "too much" and the "too little" of a variable were more closely related than either extreme to the middle. The facts which support the concept of the closeness of opposites point also to the inadequacies of many current conceptualizations of personality and to the need for concepts to describe a person more representatively.

Witkin (48) reports work in progress in an extensive laboratory-clinic study of perception and personality. On the assumption that tests which involve the body directly will be more revealing of personality than other types of tests, Witkin is employing procedures to measure accuracy and consistency of orientation where successful orientation depends upon an effective integration of clues from a number of senses. Since the body is the vehicle of the self, he reasons that information about significant personal characteristics will emerge from the study of various perceptual processes. Results of this method are not reported; its efficacy remains, therefore, to be judged.

The problem of value, long ignored by psychologists as a metaphysical problem, has in recent years received increasing recognition as an essential variable in scientific psychology. The first of a series of symposia on persistent theoretical problems in the field of personality is concerned specifically with "Values in Personality Research" (49).

Experimentally the influence of value, as a variable of personality, upon perception has been most often investigated. Current studies in this area are those of McGinnies & Bowles (50) and Vanderplas & Blake (51). Researches of this sort have a common design: (*a*) to administer a test of values (Allport & Vernon's *Study of Values* or a part of it); (*b*) to give a perceptual test with words or objects representative of the measured values; (*c*) to determine

the relationship of responses in part (*a*) to responses in part (*b*). The results of such investigations are open to the criticism made by Klein & Schlesinger (41) and others, namely, that of neglect of the person. Furthermore, even though the data of these studies support positive conclusions, one may wonder to what extent deep values are tapped in them, the real investigation of which would lead into the very personal realm of phenomenology.

McClelland & Liberman (52) present some slight evidence that need for achievement affects the recognition-time of words related to the need.

Factors of personality which act as organizers of perception and of which the individual may be completely unaware continue to be experimentally explored. Whether these are to be equated to the concepts of repression and unconscious perception remains, however, a moot question. McCleary & Lazarus (53) find that "even when a subject is unable to report a visual discrimination verbally . . . he is still able to make a stimulus discrimination at some level below that required for conscious recognition" (p. 178). This response they call "subception." McGinnies (54), in a rather similar study, finds what he considers to be evidence of the defensive character of some perceptions.

Continuing his attempt to describe the rigid person, Rokeach (55) investigates the relationship between perception-time and rigidity and concrete thinking. His general assumption is that a person is not able to remove himself from a situation sufficiently to think abstractly or to behave flexibly because of one of two reasons: either he is impatient and intolerant of delay, or the physical situation is such (or perceived as such) as to force him to hurry. Using the same technique as in his previous study, that of arithmetic reasoning problems, he divided his group of subjects into four sub-groups which differed only in amount of time-delay interposed by the experimenter between the time of presentation of the problem and the moment of starting to write down the solution; the total time allowed for solution was two minutes in every case. The differing delay-periods, 10, 20, 30, 60 seconds, were assumed to represent differences in "time-available-to-perceive." Rigidity, as measured by the number of times a subject is unable to shift from a complicated to a simpler method of solving a problem, was inversely related to time-delay; and concrete-thinking, as measured by amount of scratch-paper and verbosity in expression of answer, was also inversely related to time-delay. Rokeach interprets his results as follows (55, pp. 214 to 215):

> Some individuals, because of past experiences with frustrating situations involving delay of need satisfaction, become generally incapable of tolerating frustrating situations. To allay anxiety, such individuals learn to react relatively quickly to new problems which continue to arise. This results in consistently narrow perceptual organization. At the same time, it also prevents the establishment of abstract modes of thought since abstract thinking can only occur when there is time available to think through a problem. The inevitable consequence, it is hypothesized, is behavioral rigidity.

Personality and expression.—The most generally accepted notion in psychology today is that all behavior is motivated. Thus, for example, un-

derlying the studies reported in the preceding section is the assumption that behavior, even the simplest response, is motivated; find the springs of action and personality is revealed. To Maslow (56), this assumption is not so obvious. He suggests that there are two kinds of behavior: coping and expressive. Coping behavior, he would agree, is motivated; but expressive movements, he argues, are not. To Maslow it seems that the uniqueness of an individual is most clearly revealed and can best be studied in unmotivated, expressive behavior. Besides being unmotivated, expressive behavior is said to be largely determined by the state of the organism, being principally an epiphenomenon; it is most often unlearned, often uncontrolled or uncontrollable. Not designed to do anything, it is often an end in itself and most often not conscious. Examples of expressive behavior cited by Maslow are the beauty of a beautiful woman, the stupidity of a moron. The dichotomy of coping and expressive behavior resolves itself and is meaningless at the highest level. The ideal personality is one that is self-actualized, one, in other words, who consciously and purposely seeks spontaneity.

Maslow (57) gives us a description of the self-actualized person, and, in a search reminiscent of Diogenes, tries to find in life, in history, and in fiction individuals who exemplify the type. As some of the characteristics attributed to the self-actualized personality clearly indicate, Maslow is not afraid to stray from the conventional paths of experimental and clinical psychology. Maslow's views are decidedly provocative; but that they will meet resistance seems almost certain. To many they will appear difficult to submit to investigation and crucial test.

Johnson (58) centers his study of personality around speech. He believes that the relationship between them is so close that the two terms can be disentangled only with difficulty; neither has significant meaning in isolation from the other. In criticism of Johnson's near identification of speech and personality, it may be noted that personality has determinants other than social communication. Conceivably, two men on separate desert islands with no communication with other individuals would develop very different personalities, though, of course, the dimensions of variation in personality would be restricted in such impoverished environments. In our own culture, moreover, the relationship of which Johnson speaks does not hold all along the dimension of speech. What he avers holds principally for cases of speech pathology. Pronounced fluency is not necessarily the mark of a highly integrated personality.

The more lasting expressions of personality are considered by McCurdy (59) as valuable resources for the study of personality. The use of personal documents in psychological science has gained wide acceptance in recent years. McCurdy now tries to work out a theory and a method for the use of literature in the analysis of personality. If projection into literary work is meaningful, and if methods adequate to theory and material can be developed, then, he argues, fiction can be a nearly inexhaustible source for the study of personality. The fact that an author draws on his environment (or past environments, for there is usually a lapse of time) and colors it with his

own personality yields content which can be studied; consistency of style and themes provides form for study. Literature as a source for personality study certainly must not be overlooked, for fiction itself may be fact in psychology. But the limitations of such studies must also be recognized; paleontology is a far cry from physiology.

Berlyne (60) sees interest as the key to personality. "If we regard personality-structure as a matter of characteristic response-patterns which can be broken up into sub-wholes or 'interests,' the way is open for a study of personality in terms of motivation" (p. 194). The article presents historical notes, analyses, and definitions, and states a platform, but no clinical or experimental evidence is marshalled in its support.

The effects of success and failure.—Level of aspiration, once a lively topic in the experimental literature, is this year the object of but one investigation. Child & Whiting (61), seeking to study the determinants of level of aspiration, not in the laboratory but in the world of everyday life, asked each of 151 college students to write descriptions of three events in his life: one where there was complete frustration; a second in which a period of frustration was followed by later gratification; a third in which there was attainment of a goal with no frustration. A specially prepared questionnaire on each type of event was answered by the subjects. In the light of the answers five Lewinian propositions were tested. Clear confirmation of the two following propositions was obtained. (*a*) Success generally leads to a raising of the level of aspiration; failure to a lowering. (*b*) Shifts in level of aspiration are in part a function of changes in the subject's confidence in his ability to attain goals. The remaining three propositions were either only partially confirmed or the evidence in support of them was ambiguous.

Kelman (62) concludes on the basis of an experimental study that success and failure affect suggestibility (measured by shift in autokinetic movement) in a manner predictable by the principle of reinforcement and related principles of learning. He found, furthermore, that when they were told nothing or were led to believe that they had failed or were given ambiguous information about their performance, subjects who scored low on the ascendancy, self-confidence, and lack of nervous tension factors in the Guilford-Martin G A M I N Inventory were more suggestible than those who scored high on these factors. When, however, low-scorers were told that their judgments were correct, they were less suggestible than high scorers. This the author interprets to be the result of the increased reinforcement value of success for individuals with heightened anxiety and greater insecurity and hence greater drive. The demonstration in this study that suggestibility is, in part, learned is an important one.

In a laboratory study of failure, Katz (63) asked 70 college men to draw a human face. Following this, one half of the group were presented with simple-appearing but unsolvable problems which served to create experiences of failure for them. The other half spent an equal amount of time simply reading a newspaper or studying. Then all subjects were asked again to draw a face. The postfailure drawings could be distinguished from the prefailure

drawings in several ways: (*a*) omission of detail and distortion; (*b*) masking and inanimate expression; (*c*) blank stare; (*d*) toughness and stiffness; and (*e*) covering up part of the face. These were interpreted as expressions in the drawings of emotions and tendencies which the subjects actually experienced after failure. Thus Katz hypothesizes that the subject, in a sense, seeks to protect himself from outer threatening forces (the experimenter) by covering up part of the face which he draws and seeks to lessen his own internal disturbance by reducing the emotional expression of a face or by hardening its lines.

One very significant variable of personality, namely frustration tolerance, has proved difficult of experimental measurement. Among other problems, the question of the generality of frustration tolerance has remained unsolved. Employing the maze-block method, Thiesen & Meister (64) sought to test the adequacy of various methods of measuring frustration tolerance including eight physiological measures. Of the latter, GSR and pulse rate prove to be the most discriminating, but a multiple behavioral rating of frustration tolerance was fully as valid as any physiological index and much more readily obtained. In this study the frustration tolerance of children with high MA's and IQ's was discovered to be high, while that of children who were overly talkative in the classroom was low. For the most part, however, the evidence pointed to the relative specificity of frustration tolerance.

The frustration-aggression hypothesis continues to be a controversial issue, though no longer so hotly debated as when first formulated. In criticism of some of the studies of the Yale Group, Pastore (65) calls attention to the fact that the occurrence of aggressive responses depends on a subject's perception and understanding of the situation. The phenomenological appearance of a situation and the functional nature of an aggressive response to it are emphasized. For example, aggressive actions may express a subject's realization of the injustice of a situation or they may serve to maintain a sense of dignity or of individuality.

As his contribution to a symposium on aggression held by the Medical Section of the British Psychological Society (66), Turquet discusses factors in the early life of a child which may predispose to aggression. He suggests that a mother's expecting more of a child than he is capable of giving at the time, her unconscious conflict and guilt over the power she exerts over him, and his consequent uncertainty of affection, encourage aggression on his part. The social forces which later play upon the individual and provoke aggression are considered separately for the two sexes: The business and professional world seems to reward the aggressive man; denial of equality in many spheres engenders aggression in the woman who seeks to pursue a career; the subservience required of a woman who wants a home and family leads also to aggression. The various forms and functional values of aggression are discussed at a rather commonsense level. One misses in the discussion however, any conceptual clarification of the problem of aggression which is still so badly needed.

Personality and the mechanisms of ego defense.—There appears to be some

waning of interest in the experimental testing of psychoanalytic concepts. In part, this may be a function of the fact that the dynamic emphases in psychoanalytic theory have been extensively integrated into general psychological theory within which they now serve as a rich source of the hypotheses tested in so much of present day experimental psychology. The researches reported in the symposium, "Interrelationships between Perception and Personality" (1) were not designed to test psychoanalytic hypotheses, but they are clearly derivatives of experiments which, in an earlier day, would have served just that purpose. Yet attempts to achieve increased conceptual clarification in psychoanalytic theory and to submit fragments of the theory to experimental tests still appear sporadically. Benjamin (67) discusses certain methodological problems basic to such attempts.

Zeller (68) offers us a historical review of the literature on repression, and in criticism of previous attempts to investigate the process emphasizes the fact that "no test of repression can be considered adequate until the removal of the repression factor has resulted in the restoration to consciousness of the repressed material" (68, p. 46).

Lichtenberg (69) reviews the various conceptions of sublimation which have been proposed, but attempts no experimental study of the process.

Stoke (70) rejects the Freudian explanation of the Oedipus complex as the determiner of identification. He contends that such a view is not equally applicable to boys and girls, leaves certain facts unexplained, and, in any case, is not testable. In its place he would substitute principles which he believes more sound theoretically and more usable in practice. On the basis of experimental and clinical studies, Stoke proposes ten such principles or factors which, he believes, influence identification.

In a series of studies on the Rorschach test, Schactel (71) discusses the problem of projection in relation to character attitudes and creativity in kinaesthetic responses to the test. He suggests that man views both others and the environment automorphically as well as anthropomorphically, and that the chief mechanism for automorphism is projection. Schactel's belief that kinaesthesis is functionally close to the core of personality is similar to the views of Werner & Wapner (42) and of Witkin (48). Like Freud, Schactel defines projection as a process which occurs outside of consciousness, but he differs from the orthodox Freudian in saying that its content may or may not be conscious and in maintaining that projection may be nonpathological and even constructive.

Seaton (72) reports an experimental study of projection in which children were presented with a number of incomplete stories for which, however, multiple choice endings were supplied and from which the children were to pick the most appropriate one. There were two groups of subjects: 50 children who had been clearly rejected by their parents and a control group of 280 children. Seaton found that the rejected children did not select more parental rejecting endings than did the controls. He found also that the shy day-dreamers in the rejected group did not choose more parental accepting completions than did the other rejected children. There were, however, sig-

nificant relationships found between certain ratings of personality and the kind of projective completion which was chosen, e.g., uninhibited rejected children chose more parental accepting responses than did uninhibited control children. Also, certain differences were found to hold for both rejected children and normal controls; e.g., older children, those with higher MA's, and girls chose fewer rejecting completions than other children.

In much current literature the concept of projection is extended far beyond the bounds originally set for it by Freud. Its meaning has become so broad and so equivocal that clear redefinition of the term is sorely needed. Especially disturbing is the confusion of projection and perception which leads many writers to use the terms synonymously.

Texts and General Treatises

Hardly a book on psychology which has appeared in the last year has failed to devote considerable space to the topic of personality. Two general treatises devoted solely to the topic and published as textbooks are Harsh & Schrickel, *Personality: Development and Assessment* (73) and Cattell, *Personality: A Systematic, Theoretical and Factual Study* (40).

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SOCIAL PSYCHOLOGY AND GROUP PROCESSES

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Social psychology continues to be the meeting ground for specialists trained in many disciplines: clinicians, public opinion researchers, anthropologists, perception psychologists, personnel technicians, sociologists. To the purist this means that social psychology has no field of its own and represents no distinctive branch of science; to the old-time social psychologist it means the belated recognition that many of the other disciplines are really branches of his own subject; to the educator it means a welcome fruition of the interdisciplinary approach he has long dreamed about. No matter what the interpretation, the facts are that the personality approach of the clinician and the social-structure approach of the sociologist are among the most important vitalizing influences for research and theory in social psychology.

During the past year there has been a good balance between laboratory experimentation and field studies. Experiments upon group effects have become more sophisticated and more ingenious. They have also been more related to the variables operative in natural social situations and hence more meaningful for good social-psychological theory. The growth of laboratory experiments has not been at the expense of field studies. Surveys and field observations are more prolific than ever and a number of universities have set up cooperative arrangements for social psychologists and social scientists to conduct field research. In addition, surveys have become more theory oriented and more concerned with study design.

Social psychology is enthusiastically taking over the paraphernalia of the older sciences in its rush for theoretical models, derivations, and hypotheses. This healthy development should prove fruitful, since it will foster rigor in methods and make possible generalization through theory testing. The objection can be made that social psychology is trying to jump an inductive stage in its development as a science since it still lacks adequate empirical and descriptive facts. Real growth, however, will come from the interaction of inductive and deductive procedures. Empiricism is a sufficiently strong part of the American culture to continue to provide the necessary correction tendencies for the model builders.

Three major trends of the decade still dominate the field. (*a*) The phenomenological emphasis of Gestalt psychology has made itself felt in almost all areas of investigation. The environmental or stimulus-situation has been replaced by the psychological field. Prestige suggestion is now explained as cognitive restructuring; social norms are not statistical averages or modes but consciously shared beliefs and values; groups are not defined as common objective roles but as the mutual awareness of one another by group members; leadership exists only when the leader is perceived by the follower as

controlling the means to his need-satisfaction. Accordingly, many of the problems of social psychology can be solved in the perception laboratory. This reaction to social behaviorism is healthy and in many ways justified. The behaviorists had no valid way of taking into account the intervening variables and in the social world the intervening variables are of tremendous significance. Like all reactions, however, the subjectivist trend has gone to something of an extreme so that the intervening variables are now taking the place of independent and dependent variables as well. There is a real danger of circularity in a dominantly mentalistic approach.

(*b*) Fortunately, the Gestalt approach, with its concentration upon the psychological field, has been countered by the contributions of the clinically oriented psychologists. Their continuing research shifts attention from the present subjective state to the mechanisms of personality that were developed as a reaction to the real social world in which the individual grew up. Personality theorists remain one of the influential sources of research and system building in social psychology. Their weakness is their neglect of present social realities and their assumption that society is made up of personalities acting in parallel rather than in interaction. They are too ready to say that discrimination against groups means that there are prejudiced personalities who have been warped in their childhood training, or that group conflict means that there are too many aggressive personalities.

(*c*) The third trend is also a corrective for the subjectivism of the Gestalt school because it is sociologically derived and describes the roles and culturally prescribed behaviors of the social structure. It emphasizes present social realities and gives major attention to the roles the individual must play as the determinants of his psychology. It investigates the individual's multiple group memberships and the cross-pressures they place upon him.

Studies of Perception

Social psychology has been making use increasingly of the old psychological principle that perception is functionally selective and distortive. Research in this field is beginning to move beyond the point of demonstrating that need affects perception to the study of the conditions and principles which are operative, a knowledge of which would make prediction possible.

The results of an experiment of Pepitone (1) suggest some of the factors which must be systematically manipulated in further work. Pepitone compared a highly motivated and a poorly motivated group on their perception of three group situations, which represented different degrees of restraint and friendliness. Specifically, two groups of high school boys with differential motivation were given an opportunity to win a free ticket to a University basketball game by appearing before a three-man selection board which questioned them about their knowledge of sports. The behavior and attitudes of the board members were prescribed so that in the three situations they assumed various roles of hostility, neutrality, and friendship. The boys were later interviewed to get measures of their perceptions of which board members had the most power and gave them the most approval. The data

showed that there was facilitative distortion, that is, the subjects tended to magnify the power of the board member favorable to them. But the data also indicated that some subjects may distort in a negative manner, that is, may see the situation much worse for themselves than is actually the case. Moreover, under some conditions, high motivation may make for more accurate perception. In brief, the intensity of the individual's need, the relation of the situation to his need, the restraining or threatening character of the situation, his previous reality-tested perceptions in this area, and the nature of his personality are all important variables. The experiential and personality variables which affect perceptual selection and distortion are still not adequately identified and measured.

In their treatment of perceptual selection, Bruner & Postman (6) distinguish two opposing principles, vigilance and defense. Lindzey & Rogolsky have interpreted their findings on the identification of minority group members according to the principle of vigilance (2). They find, as did Kramer & Allport (100), that those high in prejudice are more accurate in identifying Jewish faces and non-Jewish faces than those low in prejudice.

The Suggestion Doctrine

The older concepts of suggestion and stereotypes have been under attack ever since the attempts of the Wertheimer students [notably Asch & Lewis (101)] to demonstrate experimentally that cognitive reorganization rather than a blind emotional reaction is at work in prestige experiments. Berenda has followed this Gestalt interpretation in presenting the results of experiments with groups of children from seven to thirteen years of age (3). The alleged opinion of the children's own group had much more weight than teacher's opinion. The position of superiority occupied by the teacher may create a wall or barrier rather than a vantage point for the acceptance of suggestions. Berenda also confirmed the Sherif doctrine that suggestion occurs most readily in ambiguous situations and her conclusion is that children do not show a blind and uncritical acceptance of the view of the majority. The proponents of the suggestion doctrine, however, can interpret both findings as consistent with their beliefs. In the case of the greater effectiveness of the group opinion over the opinion of the teacher, it is a matter of the source of the prestige. The greater suggestibility in the ambiguous situation does not in itself disprove suggestion. It merely permits alternative explanations.

Kelman presented additional evidence that suggestion is not a unique process; his experiments were designed to show that the usual principles of learning and reinforcement are the functional determinants of the suggestion process (4). The autokinetic situation was used and the suggestions came from confederates of the experimenter. After an initial session of judgments, one group of subjects was informed that it had been successful, another group was told it had failed and a third, or control, group received no information. In a second session, the suggestions were given by confederates of the experimenter and varied considerably from the original judgments of

the subjects. The failure group proved the most suggestible, the control group occupied an intermediate position, and the success group proved to be the least suggestible. The proponents of the suggestion doctrine will not, however, see this experiment as any more crucial than the Asch experiments. For them suggestion means the blocking out of critical or discriminating responses under emotional, fatiguing, or distracting conditions with a resulting restriction or narrowing of the field of consciousness. In the Kelman experiment, the least suggestible group received conflicting sources of information from the experimenter and from his confederate. Hence, on the basis of the suggestion doctrine, the conflicting sources of information would predispose toward a fuller awareness and the suggestion school would predict the same outcome.

The theoretical controversies about suggestion cannot be easily resolved because of the difficulty of setting up critical experiments to test the points at issue. The nature of the "suggestion" process has to be inferred from the characteristics of the stimulus situation or the resulting behavior and there is as yet no agreement on what constitutes a "suggestion" stimulus or a "suggestion" response. What keeps the controversy alive is the fact that in social life so many attitudes and reactions can readily be interpreted as blind, irrational, non-discriminating, and emotional. For example, a recent experiment of Razran in the tradition of the prestige-suggestion school found significant shifts in judgment of photographs on general liking and character when Jewish surnames were added to the pictures (5). The Gestalt approach in social psychology shifts the frame of reference from the objective observer to the psychological field of the subject. From this point of view, all responses are apparently rational and cognitively structured. The weakness here is that we are left without any discrimination between the hallucinated, delusional world of the paranoiac and the reality-based reactions of the normal individual. Perhaps the old notions about suggestion, which involve mechanisms of blocking or inhibition or the narrowing of the field of consciousness, will find their counterpart in Gestalt theorizing under such concepts as the degree of the differentiation of the cognitive structure.

Leadership

The search for the personality variables which would account for leadership was characteristic of the earlier social psychology with its trait approach and its tests of ascendance-submission and aggressiveness. The present trend, however, reflects the relational emphasis of Gestalt psychology. Leadership is a relation involving two terms and it is impossible to study the influencing agent without also studying the people being influenced. This conception, though not new, is finally affecting the research in the field. A recent formulation appears in the definition of Knickerbocker (7) who writes: "The functional relationship which is leadership exists when a leader is perceived by a group as a controlling means for the satisfaction of their needs." This definition, though an oversimplification in that there can be leadership without a conscious perception by followers that the leader is ministering to their

needs, does contain a wholesome emphasis in pointing toward the needs and wishes of the followers and away from the rigid characteristics of the leader.

An illustration of this relational emphasis appears in the field study of Maas of 22 students who acted as leaders or advisers to youth groups of a religious, ethical, social, or recreational character (8). The groups varied in their goals and activities and in their openness of structure, i.e., whether or not they were open to all comers. On the basis of autobiographical materials and the California Test of Personality, leaders were classified as projective (self-reliant, high self-esteem) and introjective (low self-esteem and higher social adjustment). Observations of group interaction were made during the second, third, and seventh months of the study. A study was made of the changes in the perceptions that each leader had of his followers; more specifically a content analysis of each leader's diary was used to determine whether he made judgmental or biased interpretations of the behavior of members of his group, or causal or objective interpretations. The findings suggest that the projective leader in an open group is under heavy enough demands to feel important in group interactions. "His need to act and change externals (rather than to modify himself) may actually be met in the open group." His perception of the behavior of others changes and becomes more causally oriented and less evaluative. In a closed group, however, the projective leader has fewer demands placed upon him by group members who can function on many things without him. Hence his needs are blocked and he perceives his followers in an evaluative rather than causal manner. The opposite situation obtains for the introjective leader. In the closed group he is under less tension and changes his perceptions toward greater objectivity. In the open group his anxieties prevent as objective a perception of the behavior of others.

In trying to apply a relational concept of leadership, the Ohio State University Research Foundation has made empirical investigations of the interpersonal relations of leaders and followers on the basis of such descriptive categories as work patterns, personal contacts, delegation of authority, and methods of working with staff (9). Hemphill of the Ohio group has carried the situational approach further by relating effective leadership to various dimensions, or characteristics of the groups in which it occurred (10). The attempt to analyze the important dimensions of groups represents a genuine advance. Social psychology has long needed descriptive variables of group functioning in addition to the usual differentiation of primary and secondary, institutional or informal, community vs. public, etc. Hemphill compiled a list of descriptive terms of group characteristics from the literature and reduced them to fifteen common dimensions: namely, viscidity or cohesiveness, homogeneity, flexibility, permeability or the ease of attaining membership in the group, polarization or the degree to which the group is oriented toward a single goal, autonomy or independence of other groups, potency of involvement of members, stability, intimacy or the degree to which members are well acquainted, degree of control of the behavior

of members, hedonic tone associated with membership, participation of members, dependence upon leadership, position of members, and size of group.

These 15 dimensions were then represented in a questionnaire by appropriate items. Over 500 respondents described their group memberships and answered additional questions about the leadership activities and the satisfaction of group members with the leadership. The results showed that certain types of leadership behavior were differentially related to the effectiveness of leadership on the basis of the characteristics of the group. For example, the making of quick decisions by the leader was positively related to judgments of his adequacy in flexible groups and in large groups but, in general, the making of quick decisions did not differentiate the high and low leaders on most of the group dimensions. The willingness of the leader to defend his group at risk to himself is relatively more important in highly polarized groups and in groups in which there is a high degree of control of membership behavior. There were, however, characteristics of leadership behavior that seemed to hold for all types of situations: namely, the ability of the leader to advance the purposes of the group, his administrative competence, his pace setting and motivating activity, his contribution to the member's feeling of security about his place in the group, and his freedom from activities serving only his own interests.

This study is frankly exploratory and yields valuable hypotheses for further research. It is methodologically weak in that all the information about group characteristics, about the leadership activities, and about the adequacy and effectiveness of the leader come from the same questionnaire. There were no independent observations of how leaders really functioned, nor of their effectiveness, nor of the types of group activity. Nonetheless, the relationships reported have considerable face validity and the patterning of results could not plausibly be attributed to any simple halo factor in questionnaire response.

In another investigation of the generality of leadership, Carter & Nixon (11) observed pairs of subjects (high school boys) in three different types of work-task situations: an intellectual task, a clerical task, and a mechanical-assembly task. In general, the subjects who took the lead and influenced their partners in the intellectual task also influenced their partners in the clerical tasks. The mechanical-assembly task, however, gave only low intercorrelations with the other two problem-solving situations. Three other criteria of leadership were employed: the rating by school officials on potential leadership ability in the task situations, a nominating technique for leaders for the task situations by the boys themselves, and an activity criterion of offices held both in school groups and in outside groups. Neither the activity scores nor the nominating criterion were related to the actual leadership scores in the task situations. The rating of school officials showed some relationship, though not of a high order. On the other hand, the nominating criterion and the activity scores were positively interrelated as were the ratings by officials.

Carter, Haythorn & Howell repeated the leaderless-situation technique on a group of college students with an increase in the number and diversity of work tasks and an increase in the number of criteria used (12). Factor analysis of the relationship between leadership in the various tasks revealed two major factors: an intellectual leadership and a leadership based upon manual skills. The nominating criterion showed some positive relationship between performance in the task situation (an average correlation of .39). The activity scores, however, had no significant relationship with other criteria, nor were faculty ratings and friends' ratings of leadership ability much more predictive. The conclusion from these studies is on the specificity side of the ledger, but does not exclude some degree of generality in related situations.

This type of careful and controlled work in laboratory situations is essential to advance knowledge of the complex problem of leadership. It is obvious, however, that it does not take into account all of the significant variables. The essence of leadership may not be the ability to influence a single colleague in a problem-solving situation. The emotional and dynamic aspects of leadership come into play in relation to the needs and wishes of potential followers. The nature of the social group is thus more complex than the laboratory pair of co-workers. Perhaps the most valuable aspect of the Carter experiments will be the comparisons among the various criteria of leadership.

A radically different approach to the study of leadership is the exploratory survey of Reid & Ehle (13). They conducted interviews with a representative sample of the residents of the city and county of Philadelphia and found few acknowledged leaders for urban neighborhoods. The majority of such leaders play a political role and are found more frequently in the poorer neighborhoods.

The older distinction between personal and institutional leadership assumed basic differences between influence based upon direct personal contact in a small face-to-face group and leadership through institutional position. The personal leader had to possess as part of his personality structure those qualities which made for influence in the small group. The institutional leader, on the other hand, could be successful through his knowledge of the techniques of administration and institutional organization. He could take advantage of the fact that there were generalized responses of acceptance or deference to the symbols of his authority.

This distinction is now receiving recognition in the attention given to the social organization in which leadership occurs. Thus, Stogdill points out that the pioneer studies of Lewin and Moreno and their followers have made progress by considering leadership as a function of the group, but that this advance has been limited to the small face-to-face group (9). He suggests the importance of paralleling this development with attention to leadership in relation to the more complex type of grouping: the social organization. For Stogdill, a social organization differs from other types of groups in that the members are differentiated with respect to their responsibilities for

the task of achieving a common goal. In discussing the attributes of organization, he accepts the traditional distinction that within any organization there are both formal and informal patterns and he further divides these patterns according to responsibility variables and interaction variables. Organizational leadership is still regarded, however, as an influence upon the activities of the organization rather than in terms of influence upon persons.

In keeping with this point of view, the Ohio State Leadership Studies have proceeded to examine organizational charts and manuals to determine the goals and structure of the organization and have supplemented this procedure with interviews with members of the organization. A modified job analysis procedure is used to determine work patterns. The informal interaction is determined by sociometric methods. The net result of this approach should be a genuine increase in the understanding of leadership as an organizational phenomenon.

What is still very much needed for social psychology in general, as well as for a knowledge of leadership, is a set of descriptive concepts for the attributes of organizations which will go beyond the old formal-informal distinction. For example, we need to direct attention to such elementary aspects of organizations as growth patterns. A rapidly developing organization which has certain goals to achieve under emergency time-pressures presents an entirely different time pattern from a stable organization which may have even passed the peak of its power. In the former case, the leadership pattern may emphasize initiative, creativity, daring and, to some extent, a rejection of traditional pathways to goals and even a reformulation of organizational goals. The organization may be one in which there is tremendous upward mobility and high motivation. In the latter case of the older, even declining, institution, the pattern may be one of conformity to tradition, an emphasis upon conventional pathways to conventional goals and even a change from the goal of the organization to the goal of efficiency as such. It is only necessary to maintain motivation at a minimum and efforts are bent toward stability rather than toward maximum productivity.

Methods for Studying Interaction and the Conference Process

Group functioning and social interaction must at some point be studied directly through actual observation of social behavior. Interview techniques deal with individuals isolated from the process of social interaction. Even group interview techniques do not reproduce the actual social situation. Measures before and after experimental manipulation still omit a description and measurement of process. Techniques for the observation and measurement of social interaction are now beginning to receive the attention they have long merited.

An attempt at a universal formula for determining the amount of group interaction has been proposed by Dodd, based upon the size of groups, their geographical proximity, their homogeneity in activity, and the rate of their activity (14). Kephart has presented specific formulas for the determination of the various possible interrelationships within a group, or subgroup com-

binations, on the assumption that the ratio between potential and actual relationships would help in the understanding of the dynamics of behavior (15). The actual units to be measured are not specified and this raises the basic problem of behavioral observations: what dimensions of behavior are to be noted or recorded by the observer?

Complete protocols, even where possible, postpone the problem of conceptual categorization for later analysis. Bales has proposed a theoretical framework for the differentiation of persons in small groups according to the four dimensions: (*a*) the differential degree of access to resources, i.e., the extent to which group members have available to them means for achieving the ends of the group, (*b*) the differential degree of control over persons, (*c*) the differential degree of status in a stratified scale of importance or prestige, and (*d*) the differential degree of solidarity or identification with the group as a whole (16). In observation, Bales records the interaction between members on a profile of 12 categories: shows solidarity (raises others' status, gives help), shows tension release, agrees, gives suggestion, gives opinion, gives orientation, asks for orientation, asks for opinion, asks for suggestion, disagrees, shows tensions, shows antagonism. How this set of observational categories relates to the theoretical framework is not clear, but Bales has worked out detailed techniques for the application of his coding scheme including an interaction recorder and the determination of coding reliability of independent observers. He has, moreover, demonstrated different profile patterns for the interactions of a group under different types of leadership and for different types of groups. He has accumulated some 23,000 scores on his 12 categories from various types of groups (17). It is possible to use the average pattern of these scores as a standard and to relate the groups showing significant variations to the conditions under which the interaction took place.

The University of Michigan Conference Research Project, under the direction of Marquis, Guetzkow and Heyns, has been investigating the group conference process for the past three years, especially as it relates to the decision-making discussions used for administrative purposes. The project has not only been concerned with the theory and practice of interpersonal behavior, but it has also been developing methodological tools for the study of interaction. Guetzkow developed a systematic set of procedures for estimating the reliability of both categorizing and unitizing operations which has general as well as specific relevance to problems of coding (18). A roles questionnaire was constructed by Heyns with descriptions of parts played in problem solving and interpersonal functions; it was found that role designations could be made reliably by naïve participants (19). It was further demonstrated that problem solving can be broken down into categories which can be reliably coded by trained observers in real life situations or from wire recordings and type scripts (20). Heyns & Berkowitz tested the sensitivity of the observational system for problem solving and found that it could differentiate between high and low quality groups (21). Consideration was also given to the problem of coding motivational factors and it was

shown that the self-oriented needs of conference participants could be rated reliably by trained observers (22). A relationship, moreover, was established between the expression of these needs and the satisfaction of the individuals with the conference decision.

In addition, the conference investigators have made extensive field observations of real life conferences in government and industry. In one exploratory study, Peterman found moderate but consistently positive correlations between the individual's sociometric score and his amount of participation (23). The satisfaction with group decisions was not related to amount of participation. The studies which followed employed three-man observational teams: individual interviews were conducted both with the conference leader and with each participant one or two days after the conference. Participants also made postconference ratings and a followup questionnaire was used one or two months after the conference.

Finally, laboratory experiments have been undertaken to manipulate the many variables which might affect satisfaction with the conference (19). In one of the laboratory experiments, participation in a leadership role by non-leaders was favorably regarded only where the leader himself was not performing these functions. Another finding was that individuals who perceived their groups as unified were more satisfied with the groups' decisions than their colleagues. In another experiment, Sperling was not able to establish any relationship between the heterogeneous or homogeneous nature of the groups and satisfaction with the group decision or willingness to execute the decision (24). The Sperling experiment revealed a "loser's" effect, namely a relationship between dissatisfaction with the group's decision and the discrepancy between the participant's original solution and the group's decision.

Group Dynamics and the Study of Small Groups

The older work on group effects was content with the examination of such global variables as social conformity and majority opinion. The present trend is toward a more thorough examination of processes at work in the group situation, a more precise definition of types of group situations, and the experimental demonstration of the effects of specific variables, as was evidenced in the University of Michigan Conference Research Project. The followers of Kurt Lewin have been the leaders in this development and they have termed the subject matter of their studies "Group Dynamics." The institutional source for this school is the Research Center for Group Dynamics, formerly located at the Massachusetts Institute of Technology, but now a division of the Institute for Social Research at the University of Michigan. In describing their theory of social communication, Festinger differentiates three of many possible sources or origins of forces to communicate: (*a*) pressures toward uniformity in a group, (*b*) forces to locomote in a social structure, and (*c*) emotional states (25). This third source has received little attention in their experimentation so far, partly because emotional expression is seen as consummatory rather than as instrumental communication. Thus, the

emotionally aroused person achieves catharsis from his expression but is not primarily concerned with communication of information to those who are present.

Pressures toward uniformity and communication.—The first major source (pressures toward uniformity) can be studied with respect to the group member communicating, the selection of the recipient, and the effect upon the recipient. It is hypothesized, then, that: (*a*) The pressure on members to communicate to others in the group on a given item increases as they perceive discrepancies among themselves on this item. Where there are no perceived differences in opinion, there will be no pressure to communicate. (*b*) With any given magnitude of perceived discrepancy, the pressure on a member to communicate to others in a group about an item will increase with the increase of relevance of that item to the functioning of the group. Verification of this hypothesis is found in an experiment of Schachter in which discussion of the same issue was made relevant for some groups and largely irrelevant for others (26). Where the issue was relevant to the functioning of the group, there were stronger forces to communicate and influence other members. There were fewer prolonged pauses in group discussion and longer individual contributions to discussion. (*c*) The pressure on members to communicate to others in the group concerning a given item increases with an increase in the cohesiveness of the group. Cohesiveness is defined as "the resultant of all the forces acting on the members to remain in the group."

An experiment by Back set up different groups of high and low cohesiveness on the basis of three sources of attraction for group members: personal liking, prestige in belonging to the group, and the promise of reward for performance in group activity (27). Regardless of the source of attraction, the more cohesive groups proceeded at a faster pace in the discussion than the less cohesive groups.

The hypotheses concerning the determinants of choice of recipient for communications and of changes in opinion follow similar lines (25):

(*a*) "The force to communicate about Item X to *a particular member* of the group will increase as the discrepancy in opinion between that member and the communicator increases." There will be a tendency, then, for people to direct their communications toward those within the group whose opinions differ most widely from their own. In the Schachter experiment, one of the "planted" subjects had the task of maintaining a divergent point of view from the norm of the group. This deviate received about five times as many communications as were addressed to the others.

(*b*) "The amount of change in opinion resulting from receiving a communication will increase as the pressure toward uniformity in the group increases." In other words, members will be more ready to change as a result of communication when the pressures for uniformity increase and, in addition, they will be the recipients of a greater amount of communication from their fellows. The evidence to support this hypothesis comes from an experiment by Festinger & Thibaut, in which three degrees of pressure toward uniformity were created (25): high pressure by instruction that the

experimenter was interested in how a group reached a unanimous decision; medium pressure by instruction that their score would be based upon the proportion of members who recommended the correct solution; and low pressure by not giving any instructions about reaching a unanimous or majority decision. The greatest change as a result of communication occurred in the high pressure group and the lowest change in the low pressure group. The real question that can be raised about this hypothesis concerns its circularity. How could unanimity be achieved in one group and lack of unanimity in another group without greater change taking place in the first group? In other words, if uniformity is to be created in a group, change is necessarily implied.

(*c*) "The amount of change in opinion resulting from receiving a communication will increase as the strength of the resultant force to remain in the group increases for the recipient." The person who wishes to remain in a group will be more receptive to change through communication with the group members than a person less eager to stay in the group.

A laboratory experiment by Back (27), previously mentioned, created high and low cohesive conditions for different groups. With high cohesiveness, the forces on a member to remain in the group would be stronger than under conditions of low cohesiveness. In all the situations in this experiment, the members of the high cohesive groups influenced one another more than was true of the members of the low cohesive groups.

Further confirmation comes from a study of a housing project (28) by Festinger, Schachter & Back. A correlation of .72 was obtained between the attractiveness of the group for its members and the effective maintenance of a group standard relevant to the functioning of the group.

(*d*) "The amount of change in opinion resulting from receiving a communication concerning Item X will decrease with increase in the degree to which the opinions and attitudes involved are anchored in other group membership or serve important need-satisfying functions for the person." This hypothesis takes account of the fact that an individual may have multiple group membership and that he has other needs apart from those related to the immediate group situation.

Forces to locomote in a social structure.—The general hypothesis here is that people who are motivated toward upward mobility in the group will tend to communicate with those above them in the organizational structure. In one field study conducted by Back and associates (25), items of information were planted with people at various levels in the hierarchy of an organization. Eleven of the 17 acts of communication that were recorded were directed upwards in the hierarchy. In an experiment by Thibaut (25), two groups were created, one of high status and privilege, the other of low status and privilege. The groups were brought together to play a game in which low status members functioned as servants. The communications addressed to the high status group increased as its prestige position became clearer; and the communications downward decreased.

On the other hand, hierarchical structures contain restraints against

communication. The very existence of status differentials may operate to prevent ease of communication. In an experiment by Kelley (25), experimental groups were set up with a two-level hierarchy which engaged in problem-solving tasks. The control groups performed the same type of task but contained no status differential. Communication was greater between the subgroups under controlled conditions than between the subgroups in which there was a status differential.

Important next steps in this research program should consider the relationship of the forces toward communication and the restraining forces. It would be valuable to know under what conditions the restraining forces are dominant and under what conditions the forces to communicate are more important.

These hypotheses are not intended to exhaust the significant variables in social communication and influence, but they illustrate the experimental approach of the Group Dynamics school. The ideal outcome of this research would involve further studies in which the differential interaction of variables is measured so that equations could be written which would predict, for specified conditions, the resulting type and manner of communication.

Another contribution of the Group Dynamics approach is the devising of experimental group situations which depart from the artificiality of the usual laboratory setting but which still isolate given variables. For example, Schachter asked for volunteers for groups which supposedly had real functions (26). Students were invited to join an editorial club, a movie club, a radio club, and a case study club. In each club were three paid participants who acted as stooges for the experimenter, unknown to other members of the group. The case study club, in its first meeting, began by reading an abbreviated version of the life history of a juvenile delinquent. Discussion of what should be done with the delinquent followed. One paid participant in the group took the position of deviant and maintained it throughout the discussion. Another paid participant took the modal position. The third paid participant started as a deviant but allowed himself to be influenced to the modal position. Sociometric ratings were also obtained. The participants playing the deviate role tended to be rejected by the group in the sociometric rating. There was more rejection of the deviate in the high than in the low cohesive groups. The group members were not aware that the clubs had been set up for experimental purposes, nor were they aware of the manipulations going on. In this experimental situation, the advantages of a realistic life-situation with valid group processes at work were realized under conditions making possible the rotation of experimental variables.

Action Research

Besides experiments on small groups in laboratory settings, the followers of Lewin are continuing their interest in action research. Here the concern is with the real life situation, because as Lippitt writes: " . . . for fruitful study the scientist must be able to work with genuine groups which have a group history, group goals, established interpersonal relations between the

members and a given place in the community structure" (29). Action research generally implies working with action groups or practitioners, observing the change process and evaluating the effectiveness of the change program. This conception is ingenious in attempting to gear research into ongoing social processes. It makes feasible the support and the acceptance of social psychological research by practical operators. Its success depends upon full collaboration between researcher and practitioner, even to the point envisioned by Lippitt of the two interchanging their roles (30). And the group dynamicists have developed a rationale and are attempting to implement it with a training program which has reached its most complete development in the National Training Laboratory at Bethel, Maine.

Whether the rationale and the practice of action research can solve all the ethical and scientific problems involved in the union of practitioner and researcher is a subject of debate. The ethical problems concern the role of the social scientist in accepting the goals of the practitioner and helping him to achieve them. The scientific problems concern the limitation of research that is geared to practical needs and raise the old question about basic versus applied research. Moreover, the emphasis in action research is on the techniques for changing people in small group situations and there is a neglect of institutional factors which make for change or stability. Two answers have been suggested by action researchers to meet the ethical issue. Cook, in a symposium discussion with Cattell at the 1949 meetings of the American Psychological Association, demonstrated that there were common social goals accepted by all groups in our society (31). Thus, in doing action research in an established housing project to minimize tension and friction among the various ethnic groups, the social scientists are not taking sides on a social issue. Similarly, in the public health field, research on social action to lessen disease rates is in the public interest. Another answer suggested by the group dynamicists is that if democratic participation techniques are employed as an intrinsic part of action research, the method provides the solution to ethical problems. If the problem being worked on is controversial, the change that is sought is one which the interested parties work out for themselves. French & Zander propose a collaborative effort on the part of management, union, and scientist in the study of labor-management relations (32). A related justification for action research assumes an analogy between the healthy community and the healthy individual. Thus, Curle sees action research as a form of social psychiatry in which the objective is to give the community more power to remove the conditions which are frustrating it or to tolerate those frustrating elements which cannot be directly removed by developing their positive components (33).

This second type of rationale encounters practical obstacles in that it is extremely difficult to involve opposing interest groups in collaborative effort. Even if the action researcher could link a specific union and management in a cooperative research project, this might be at the expense of other groups in the community or of other companies and workers in industry. In general, action research must take more account of the fact that even political de-

mocracies are made up of competing interest groups. Men are organized in their struggle for material rewards and for power and prestige in interest groups. The resolution of such conflicts cannot be achieved where either competing group would lose substantially by the resolution.

On the scientific side, there are two major limitations to action research. It does not seek for the manipulation of the significant variables which are important for scientific theory. Specifically, one important determinant of behavior and attitude is the social organization or social structure in which it occurs. This structure is, of course, a pattern of interrelationships of people based upon mutual expectancies and dependent in part upon a system of sanctions. But the variations in this structure are not attacked directly by action researchers. They accept the context determining the behavior of people and try to modify small groups who seek their aid on specific problems. Action research stems from group social work, its conception of field experimentation being limited. Adequate recognition is not given to the fact that field experimentation can attempt the direct manipulation of the important substantive variables for the control and prediction of social behavior. The methods for producing change are not necessarily limited to training people in the democratic conduct of small groups. For example, in a hierarchical structure it is possible to change the character of the organization by decisions at the top of the structure which can then be implemented with training at lower levels.

The second major limitation of action research concerns the interchanging of roles of practitioner and researcher. One of the greatest obstacles to a genuine social science has been the fact that we are part of what we study and are very much the creatures of our culture in habit of thought and in conception of problems. Hence, our practical interests color our approach to problems and applied research outruns basic science. Most social science findings are outmoded very quickly after their discovery, because they are not the report of the fundamental processes at work. Now, in action research we are breaking down the barriers between scientist and practitioner so completely that the immediate cultural determination of research becomes greater rather than less.

Though action research can be criticized for its narrow character, it nevertheless represents an important and fruitful movement in social psychology. In spite of the lack of the manipulation of important variables, it can build up over time a great deal of knowledge about group change. Its marriage with practice, though subject to the risks of a marriage of convenience, can also serve to protect social psychology from long excursions into sterile and unrealistic social theory.

To develop knowledge about group processes and to train practitioners in skills for dealing with people, the Research Center for Group Dynamics in collaboration with the National Education Association has been conducting the National Training Laboratory at Bethel, Maine, for the past three summers. This year, the Carnegie Corporation has given the Laboratory a grant to make possible research throughout the year on the data accumulated at

these training sessions. The staff of the Laboratory consists of social psychologists, social scientists, and psychiatrists; the students are personnel and management people from industry and government, group workers, and educators. The general rule is to have a team of two or three people from an organization so that they can reinforce one another when they return from Bethel to the home agency. The procedure at Bethel follows the OSS assessment formula in one important respect: the incoming members leave behind them all signs of their status and their customary roles. The sessions are devoted in part to group introspection. The members of a group spend considerable time on observing their own group process and so may become more sensitive to the dynamics of interpersonal relations. Another general important aspect of the training comes from the frustration of most practical operators when in a group in which their customary skills of group manipulation no longer work. The group is critical of any attempt at manipulation and insists upon methods which are democratic and representative of its wishes. The practitioner who can survive the frustration and learn new ways of dealing with his fellows may undergo something of a personality conversion.

An example of action research is described in Lippitt's *Training for Community Relations*, a report of a training experiment of Connecticut community leaders and workers interested in the reduction of intergroup tensions (34). The project was instigated by professional leaders in the state who had a program in progress but were eager to make it more effective. After consultation with social scientists, the leaders felt that the best method to help the local communities would be to make them a maximum help to themselves. Accordingly, a workshop was set up to provide a two-week training program for community leaders. The training program included sessions devoted to achieving a common understanding and acceptance of workshop procedures, defining curriculum content through group thinking and action, diagnosing the human relations problems to be solved, and practice in human relations skills. The emphasis throughout was the development of the group through its own resources and through its own thinking and action.

The effects of the workshop training were evaluated six months later in the community situation by follow-up interviews with trainees and with community observers and by observations and records of the state committee. There was a striking increase in level of activity on the part of trainees. Before the workshop, 18 per cent of the group were involved in eight or more activities aimed at improving intergroup relations. Six months after the workshop, 71 per cent were involved in eight or more activities. This self-report was confirmed by the independent observations of co-workers. The content changes were in the direction of a broadened view of the problems of community relations, more sophisticated planning of action strategy, personal changes in prejudiced attitudes, new confidence in own potentialities, heightened sensitivity to a wider range of intergroup tensions. The greatest increase in level of activity was found in the members of the community training delegations who developed into cohesive units during the training period. Where there were community delegations which showed less

cohesiveness in the workshop there was less of a gain in activity level. But the gain was greater even for these weak units than for members who came to the workshop as single representatives of their communities. The trainee was assigned an influence potentiality score based on the power position he held in his community. The people with high influence-potential tended either to be very receptive to training or very resistant to it. Though the high and low influence potential people did not vary in level of activity before training, the high group showed a greater increase in activity after training.

Intergroup Tension

Two opposing conceptions still dominate research and theory on problems of prejudice and intergroup tension. The one conception sees discrimination and prejudice in personality terms as a substitute outlet for the hostilities and aggressions created by frustration. The frustration-aggression hypothesis may be at the simple level of adult frustration and would attempt to correlate any current deprivations with degree of prejudice. At a deeper level this theory can be elaborated in terms of childhood training. Parental rejection or parental domination can produce repressed hostility which is readily displaced toward such targets as minority out-groups. This approach has been elaborated the most fully by the Frenkel-Brunswik school (35).

The opposed conception sees prejudice as a function of the social structure in which people must necessarily work out their goal patterns. This point of view appears in one form in the doctrine of social norms. Individuals, as members of a culture or of a group, take on the existing values or norms of the group. Children learn prejudice, as Hartley has pointed out, not from contacts with Negroes but from contacts with the culture norms about Negroes. To play his role as a member of a group structure, the individual accepts the values of the group as his own. In another form this approach emphasizes prejudice as the outgrowth of a competitive struggle between groups for material and psychic income. Thus, the Negroes were brought to this country for purposes of exploitation in the first instance, and their relegation to inferior economic roles still has an exploitative aspect to it. Similarly, quota systems in medical schools against minority groups may not be the outgrowth of repressed resentment toward parents on the part of the medical profession but may function to keep desirable jobs in the hands of the majority group.

The thesis here hits directly at the frustration-aggression hypothesis which basically asserts that the victim of prejudice is a scapegoat of displaced aggression. Thus, the aggression and its direction bear no relation to the nature of the target and have no problem solving qualities. Without invoking the earned-reputation theory of prejudice, it should be pointed out that the aggression toward minority groups may derive not from frustration but from the advantages to be gained from such aggression. Kardiner has shown that the Comanche Indians lived by aggression and war, not because their childhood training frustrated and repressed them, but because a warlike

pattern of raiding and confiscation in their particular circumstances paid off handsomely (36).

The evidence which would permit a definitive answer to the claims of the two points of view is not yet available. On the one hand, it cannot be denied that it is possible, under some conditions, to obtain positive, though not very high, correlations between personality patterns and prejudice. Thus, Rosenblith repeated the Allport-Kramer study in nine colleges in South Dakota and confirmed their findings that prejudice tended to go along with: (*a*) clinging to parental patterns, (*b*) jungle philosophy in which the world is held to be basically evil, (*c*) disciplinarian or authoritarian outlook, (*d*) fear of fraud and trickery, and (*e*) lack of sympathy with the underdog (37). Some of these results can still be interpreted in other than personality terms, since the values which go with prejudice may be part of the same group pattern of norms of which prejudice is also a piece.

The studies of the Frenkel-Brunswik group, however, are aimed more directly at the personality variables associated with prejudice. Their investigations report that people scoring high on ethnocentric scales have rigid, constricted personalities and undifferentiated egos with a narrow range of permissible experience. There is evidence among them of repression of hostility and the clinical interpretation is that, in general, the high scorers are dominated by castration anxiety and show anal character traits of hostile rejectiveness, retentiveness, and reaction formation. The implication is clear, moreover, that the highly prejudiced are closer to a pathological syndrome than the low scorers. In fact, Levinson writes: "It seemed to all of us . . . that there were personality trends in the high scorers which would make them more prone to develop psychotic manifestations, while the low scorers seemed to tend more towards neurotic disturbances" (35). This is, however, a hypothesis and the results from a group of patients coming into a clinic for help do not support the hypothesis. The evidence for personality patterns associated with ethnocentrism in the California studies comes only from groups of people selected from the high and low ends of the prejudice scale.

The use of extreme groups has yielded similar findings by other investigators. Himelhoch compared the 20 high scorers and the 20 low scorers on fascism and ethnocentric scales from a sample of 140 college students; on the basis of Rorschach protocols he found that the former group showed significantly more repression of hostile impulses and sexual impulses, more emotional immaturity and greater lack of insight (38).

Rokeach's report of the relationship between ethnocentrism and rigidity of character structure has been criticized by Luchins (39). Not only does Luchins object to the class theoretical approach of the personality theorists, he also takes Rokeach to task for generalizing a relationship between the two variables of ethnocentrism and rigidity on the basis of small differences between groups on measures whose validity has not been established. His most telling point concerns the small differences found. Rokeach's rejoinder calls attention to the differences satisfying statistical criteria of significance (40) and to the confirmation of his findings in the experiments of Christie (41).

The validity question raised by Luchins may mean that the variables are not correctly described, but the fact remains that relations have been found between distinctive types of measures.

There is corroboration of the frustration-aggression approach in the work of Bettelheim & Janowitz among veterans (42). Ethnic intolerance was related not to objective deprivations, incidental to service, but to subjectively felt deprivations. But objective, downward mobility, as measured by changes from prewar to postwar position, was significantly related to prejudice. Similarly, apprehension about future unemployment was associated with ethnic intolerance.

Material which raises problems for the personality and frustration theories is illustrated by the studies of Lindzey (43) and Morse (44). Lindzey set up four propositions for testing which were derived from the frustration-aggression hypothesis. The frustration experience included 10 to 12 hr. of food deprivation, taking a blood sample in such a way as to cause considerable pain, deprivation of urination facilities and failure at an assigned task in a group situation. Two propositions were confirmed, namely the displacement of aggressive tendencies on non-instigating frustration objects and a greater feeling of subjective frustration on the part of the subjects high in prejudice. But two other propositions were disproved. The high prejudice subjects showed less tendency toward a displaced aggression than did low prejudice subjects. Moreover, the high-prejudice group did not show any more evidence of outwardly directed aggression than did the low-prejudice group. Lindzey's experiment, though it does demonstrate a scapegoating mechanism as a result of frustration, challenges the assumption that this mechanism is the basic explanation of minority group prejudice. If it were, his prejudiced subjects should have shown more of this mechanism under frustration, not less.

Similarly, Morse found little support for the frustration-aggression hypothesis in a study of anti-Semitism (44). She has distinguished among behavioral exclusion from groups and organizations, verbalizations derogatory to Jews, and the tendency to withdraw from contacts with Jews. No relations appeared among personality insecurity, circumstance frustration, projected personality frustration, and prejudice with the one exception of personal insecurity and withdrawal. Behavioral exclusion was related, however, to emotional involvement in the nation. Morse follows F. H. Allport in theorizing that prejudice is a function of involvement in a collective group structure and cannot be understood primarily as a personality mechanism.

The conflicting evidence suggests that the problem of intergroup tension is one of multiple causation. The central issue may be whether the prejudiced personalities of the constricted type, with rigid and authoritarian character structures, represent the hard core of an intolerance movement or merely the lunatic fringe.

In an exploratory investigation of 45 residents of New York City who had changed their attitudes toward minority groups, Watson (45) obtained data confirming a multiple causation hypothesis. In half the cases, the changes

were isolated in nature and did not seem to involve any structural change in personality. In some of the isolated changes, the related factor was a move to a new community or group and conformity to its mores. On the other hand, there was confirmation of the Kramer-Allport findings that the prejudiced people were authoritarian, distrustful, etc. The attitudes of the sample, however, seemed to be more embedded in the context of liberalism-conservatism than in compelling psychological needs.

Cross Pressures or Conflicting Group Memberships

Even the staunchest supporters of the reality of the group must take account of the fact that the same people belong to more than one group. Frequently these groups may overlap in their competing demands upon their members and it becomes impossible to predict group behavior without a knowledge of the loyalties, needs, and perceptions of individual group members. The spectacular political example of cross pressures occurred in the 1928 contest between Alfred Smith and Herbert Hoover. The southern voter had to choose between his traditional Democratic allegiance and his religious and social loyalties. The conflicts in second generation Americans between the norms of their family and their ethnic group and the norms of the new culture were a favorite subject of study a generation ago. Every minority group has the same problem in that its members absorb some of the values of the dominant groups in the culture. Hence arises the problem of the anti-Semite in every Semite and the prejudice against foreigners among those of foreign origin. This cross pressure phenomenon has been institutionalized in some of our social organizations. For example, the non-commissioned officer must have membership among his men to be effective and must also be a representative of the officers above him. Similarly, the foreman in industry is both a representative of management and a representative of his work group. Even rank-and-file workers are frequently under the cross pressure of membership in the company and in the union. This area of overlapping group membership is a most promising field of investigation for social psychology, since it combines the study of individuals with the sociological emphasis upon social patterns.

In the industrial field, Willerman has reported an analysis of findings from one of the studies in the research program of the Survey Research Center of the University of Michigan in the area of human relations (46). The study dealt with a unionized plant with a long history of good labor-management relations. In this situation, most workers identify with both management and union. The most highly-identified management workers, however, are more hostile to the union than the highly-identified union workers are to management. Most workers, though, tend to preserve their dual allegiance by minimizing the possible conflicts in their perceptions of the situation. They see management and union as wanting essentially the same objectives and they see no real reasons why the union and company cannot cooperate to work toward their mutual goals.

In accounting for political attitudes and voting behavior, Lazarsfeld

has made the concept of cross pressures widely known (47). Both in the Erie County and in the Elmira panel studies, group affiliations of family, religion, political party, and social class have been related to voting behavior. In the Elmira investigation, Dinerman has shown that 13 per cent of the pre-election working class supporters of Dewey deserted to Truman on election day (48). One of the best predictive measures for individuals under cross-pressures is a knowledge of how their best, personal friends will vote. The Elmira data indicate that sociometric ratings correlate well with social and political attitudes and voting behavior.

Cross pressures have been described by Stouffer as the simultaneous roles which one person is expected to play by virtue of membership in groups with conflicting norms (49). To explore the dimensions of the problem, he asked undergraduates to indicate the mode of behavior they would follow if, while proctoring an examination, they noticed a student copying from his notes. Other questions dealt with the expectations of the authorities and of fellow students about the course of conduct to pursue. Still another question asked what they would do if the cribbing student were a close personal friend who had worked hard and desperately needed a good grade. Finally, the students were asked about their proctoring behavior, first, if there were very little chance that either the authorities or students would find out about it and second, if only the authorities would learn about it. The results showed that only a small minority escaped the conflict by regarding the range of approved acts as identical from the point of view of both the authorities and the students. A larger minority saw no overlap in the range of acts approved by both groups. The great majority, though appreciating the differences in the norms of authorities and students, did find at least one act acceptable to both groups. Stouffer's conclusion is that the variability of his findings suggests some revision of the common assumption that a social norm represents a point or a very narrow range of behavior on either side of a point. The range of permissible behavior is often broader because of the lack of complete agreement in the expectation of all group members and because even common expectations allow some variation. "For it may be the very existence of some flexibility or social slippage—but not too much—which makes behavior in groups possible" (49).

Group and Organizational Morale

The motivational problems in organized group activities which relate to the maintenance of the organization and to its effective functioning are frequently referred to under the heading of morale. Thus, morale can be defined as the degree to which the members of a group, or organization, are meaningfully motivated toward a group goal. The term "meaningfully," in this definition, takes account of the socially accepted pathways toward the goal. The German literature in its distinction between Haltung, or behavioral morale, and Stimmung, or subjective morale, suggests another important aspect of group motivation. If the behavior to accomplish the group objective is a function of external sanctions, we do not generally regard it as evidence

of high morale. For example, the German civilians in the last war, even under the stress of military reverses and heavy bombing, maintained a good level of industrial production and general contribution to the war effort (50). This was more a function of the police state and the Gestapo controls than of internalized motivation, so that when defeat came there was a complete collapse of all resistance.

In spite of the importance of the motivational phases of social organization, most of the research in this field has been limited to wartime or industrial problems (51). Recently, however, the broader implications of morale have been recognized. The Michigan Survey Research Center is in the third year of a ten-year program which has as its objectives the discovery of the conditions in group organization which make for a high level of group productivity, or group functioning, and a high level of satisfaction of group members. More specifically, the program calls for studies of the principles of organizational structure, of effective leadership, of interpersonal relations within the organization, and a more detailed knowledge of the types of motivation in a group structure. Thus far, the program, with the aid of the Office of Naval Research, has included studies of government agencies and industrial companies, but the plan calls for the study of political, educational and civic, and voluntary groups as well.

The findings to date suggest that the commonly accepted theories of administration and management are in need of substantial revision (52). These theories assume that social organization can be set up on the analogy of a machine with a high degree of specialization of function, heavy centralization, a vertical rather than a horizontal structure, a limited span of control for the various levels of intermediate management, strict man-to-man accountability down the line, and precise job definition.

In studying high and low producing groups matched on type of work, the Michigan Survey Research Center found little corroboration for these assumptions. The high groups were under supervisors who were not under strict accountability to their superiors and who, in turn, gave their employees more leeway. These supervisors were not dominantly oriented upward in the hierarchical structure, but had a downward orientation of understanding and of motivating the people under them. Precise job definition seemed to be followed less in the high groups with a greater pride in work group and a greater group responsibility. In brief, the traditional management theory presents a logical framework of organizational structure but ignores motivational factors. By its stress on extreme specialization of function, it destroys the meaningfulness and reward of the job itself. By its machine pattern of command, it destroys the rewarding interpersonal contacts between human personalities. To restore motivation to the organization, other devices have been superimposed upon the structure. For example, suggestion systems are employed to increase communication upward, sometimes at the expense of the more natural employee-supervisor communication. External rewards in the form of benefit systems and recreational programs have been added.

The research findings suggest that these external benefits help to produce a favorable company morale in which employees are interested in staying with the company but they do not necessarily increase productive effort.

Similar results appear in a continuing series of morale studies conducted by the Sears, Roebuck Company under the direction of Worthy (53). The Sears stores vary in size and in complexity of vertical structure. Higher morale and higher productivity are found in the smaller organizations with fewer layers of supervision, a minimum of job and departmental specialization, and loose, simple, informal structures.

> In organizations characterized by many levels of supervision and elaborate systems of controls, the individual not only has little opportunity to develop the capacities of self-reliance and initiative, but the system frequently weeds out those who do. . . . In the more simple types of organization structures, where management has been effectively decentralized, an executive accomplishes results and moves to higher levels of responsibility chiefly to the extent he is able to secure the willing, enthusiastic support of his colleagues and subordinates. . . . The outcome is not only a higher level of accomplishment, but at the same time, a more satisfying type of supervision and a higher level of employee morale.

Confirmation of this point of view is also furnished by the laboratory experiments of Deutsch on cooperative and competitive groups (54). Groups in the cooperative situation were told that their reward would be based upon the showing of the group in comparison with other groups, whereas in the competitive situation the group member was told that his reward would be determined by his own relative contribution to the solution of the problem. The results showed better quality of product in the cooperative group and more productivity per unit of time. In addition, there was greater friendliness and better overall morale in the cooperative situation. The theme of precise job definition in the machine theory of organization overlooks the possibilities of higher motivation when members have responsibility for the functioning of the group.

In his studies of organizational morale, Whyte has developed an ingenious model for the analysis of organizational functioning in terms of patterns of social interaction (55) and has carried out intensive anthropological studies of both union and management in a number of industrial concerns. He distinguishes among eight patterns of functioning of organizations on the basis of the origination of actions, specifically the source of origination, the amount, and the direction. Where management preserves a tough policy toward the union, there is no reciprocity of interaction between management and union at any organizational level. At various levels in the union structure, actions originate which tend to move up the union hierarchy where they can be exerted with maximum force against top management. In the case of union-management cooperation with adequate adjustment of foremen within the management structure, there are patterns of reciprocal interaction at all levels. Moreover, there is an upward as well as a downward flow of communication in both management and union hierarchies. This

model can be used to explore both the total pattern of relationships and possible trouble areas where reciprocal interactions have broken down at a specific hierarchical level.

Social Structure and Social Stratification

Two lines of inquiry are converging in the study of the structure of American society. One line of investigation is the anthropological study of communities and has reached its highest point of development in the work of Warner and his colleagues in their studies of Yankee City and Jonesville (56). There has been some deference to quantitative method by the Warner group but the essential approach is the interpretative understanding of the social pattern of a small community.

The other line of development, still in its pioneering phases, employs systematic quantitative procedures and sampling techniques. This second approach is illustrated by Centers' analysis of social class which is based upon interviews with a cross-sectional sample of the nation (57). In addition to socio-economic status as one of the major dimensions against which to analyze attitudes, Centers used measures of class consciousness, of the perceptions of individuals of the occupational groups belonging to given classes, and a conservatism-radicalism scale. The data were also analyzed in terms of other variables to show the significance of social class. For example, it was found that American women are very similar in their class consciousness and social attitudes to American men (58).

The problems in the use of public opinion surveys to obtain data and establish relationships about social class have been brilliantly summarized by Kornhauser. He calls for: (*a*) a better phenomenological approach rather than forced identification with one of several categories, (*b*) more thorough exploration of sub-groups, and (*c*) proof that reshuffling into other patterns will not yield as good determinants of attitudes as occupation or class identification (59).

Indirectly, the continuing studies for the Federal Reserve Board by Katona and his collaborators are building up a body of facts about the economic basis of social structure (60). The studies concern the distribution of income, the savings, and the economic habits of the American population. These surveys of the economic behavior of the individual do not yield the rich interpretative accounts of the interaction of social classes furnished by the Warner group, but they achieve a precision in measurement and in sampling which permit generalizations for the nation and its economic subgroups with respect to changes in income, in liquid assets, in buying habits, in savings habits, in economic expectations, etc. (61, 62).

The accumulation of empirical findings in this area has led to a critical examination of the concepts of social class and social stratification. Gordon, in summarizing the uses of the concept of social class, points out the omnibus use of the term and stresses the need for demonstrating that class indicates divisions which show group rather than individual differences and which determine to some known extent the social relationships of the groups involved

(63). He proposes five basic factors according to which the population may be horizontally stratified: (*a*) economic power—income or wealth and occupational role, or relationship to the means of production; (*b*) status ascription—corporate class consciousness, generalized class awareness, competitive class feeling, felt or latent status; (*c*) group life—the extent to which class provides a social system within which members have their intimate and meaningful social contact; (*d*) cultural attributes—patterns of consumption, dress, attitudes toward sex, the family, patriotism, etc.; and (*e*) political power. Investigations should specify which factor or factors they are concerned with and employ the necessary measures to substantiate their interpretations.

Hatt raises further methodological questions (64). Is the major criterion of class to be subjective in terms of perceived similarity of fate or consciousness of kind, or is it to be some objective measure of income or economic role? Is social class to be approached at the community or the societal level? In the community approach of Warner, the basic procedure involves a community reputation analysis in which the number and names of classes recognized in the community are supplied by informants. Individuals are assigned to these class positions by their reputation in the eyes of the informant, or by his statements about their characteristics, their activities, or their background which have class value. Hatt suggests that once high correlations between simple measures of income, source of income, and occupation are definitely established with Warner's more complicated index of evaluated participation, these measures can be used interchangeably.

Warner's work itself has received critical scrutiny by Pfautz & Duncan who challenge the methodology on the following grounds: (*a*) the evaluated participation index could never be checked by an independent investigator because the criteria for the scales are not made explicit, (*b*) no reliability measures are furnished for the indices, and (*c*) there is no adequate consideration of problems of sampling (65). On the conceptual side, the Warner approach is taken to task for dealing with the phenomenon of prestige as if it also embraced the distribution of political and economic power. Finally, the danger of treating an anthropological study of the small community, as if it represented a microcosm of the nation, is emphasized. These criticisms indicate that real progress will result when the insights and empirical material of the Warner approach are combined with systematic methods of quantification and measurement.

An interesting application of the Guttman scaling technique to occupational prestige and social stratification has been made by Hatt (66). The original data comprised the ratings of 88 occupations on the basis of the general standing of the occupation in a 1947 nation-wide survey made by the National Opinion Research Center. The continuum derived from these scores, when tested by the Guttman scaling technique, did not even yield a "quasi-scale." When, however, the jobs were broken down into occupational families, it was possible to scale the responses for the subgroups. The occupational subgroups yielding such scales were the professions, politics, business, recrea-

tion and art, agriculture, manual work, the military, and service. The implications of the findings are clear, namely, that there is agreement in our society about the prestige positions of jobs within the occupational family, but little consensus about the relative ranking of occupational families. These results raise questions about the assumption that there is a perceived or recognized class structure in terms of prestige.

Central to any theory of social class is the factor of social mobility. Complete lack of mobility denotes a caste system, whereas complete mobility invalidates the concept of class stratification. It is of interest, therefore, that there are no comprehensive studies of class mobility in our society. McGuire has estimated that there is at least a 20 per cent upward mobility in the United States during a generation and reports a 25 per cent upward mobility in Jonesville (67). Since the data for these figures are not given, they cannot be accepted as more than interpretative estimates. Fragmentary investigations have appeared from time to time which suggest the possibilities for further research on this problem. One recent study compared the mobility in the legal profession for three geographical areas, the northeast, the midwest, and the southwest (68). In the northeast, 55 per cent of the lawyers had fathers either in the professional, managerial, or proprietary groups. In the midwest, the comparable figure was 71 per cent and in the southwest 63 per cent. In the northeast, moreover, 10 per cent of the lawyers came from laborers or service groups, whereas the comparable figure for the midwest was 2 per cent and for the southwest zero. The hypotheses are suggested that greater professional mobility exists in the northeast because of the greater accessibility of education, the greater barriers in the basic industries and business, and the greater number of upward-mobile, second generation Americans.

The relations of class structure to the overall social pattern have been examined by Merton with respect to "anomie" or normlessness (69). Merton points out two important elements of social structure: the common goals and purposes of the culture shared to some degree by practically all its members and the institutions or forms which define the paths toward these goals. The cultural emphases upon these two elements may vary independently. One society may concentrate heavily upon the goals themselves and give little attention to the institutional procedures for reaching them, thus exemplifying one extreme of a malintegrated culture. The other extreme neglects the basic purposes and emphasizes conformity to institutional means; the result is a rigid, "sacred" society, with little adaptability. American society is of the former type with much greater pressure toward the goal of monetary success than on the institutional means for reaching the goal. The class structure is not rigid enough to prevent an upward identification and aspiration or open enough to permit free movement. Within this framework there are great strains toward deviant behavior and "anomie."

An imaginative and daring application of psychological measurement to culture patterns has been made by Cattell, who has utilized factor analysis

for the determination of the major dimensions of cultures (70). Seventy-two variables were selected for 69 nations. They included dimensions regarded as essential for characterizing groups by authorities in special fields, as well as characteristics dictated by theoretical interests for testing hypotheses of Freud, McDougall, Sorokin, and Unwin. Twelve dimensions emerged, varying from such factors as size to "bourgeois philistinism." The basic question raised by this interesting application of statistical techniques to very complex material concerns the nature of the original data and the units of measurement employed.

Propaganda and Its Effects

One school of thought gives high priority to the amount and intensity of propaganda in the development and modification of people's beliefs and actions. It assumes that in a complex society people have little access to information about social events in their own daily experiences. Hence the propagandist can affect their beliefs and attitudes by gaining their attention through noisy headlines and can win acceptance by sheer repetition of emotionally loaded appeals.

An opposed school gives priority to the basic needs of people and sees change in their ideas and attitudes as emanating from changes in their environment in relation to these needs. It is popular for the adherents of this point of view to cite the negative correlation in the last four presidential elections between the amount of propaganda and the outcome. For example, in 1948 the victorious presidential candidate received the support of only 16 per cent of the daily newspapers in the United States, representing but 14 per cent of the total daily circulation. As MacDougall has pointed out, however, these historical observations are inconclusive (71). Without heavy newspaper support, the Republicans might have received fewer votes. Moreover, even though the papers supported the Republican candidates editorially, the Democratic candidates, as public officials, received a great deal of space in the news columns. Moreover, Wilkinson & Hart have found no significant correlations between declining business activity and a decrease in votes for the party in power (72). Such a relationship is often assumed and is used to support the thesis that economic factors rather than propaganda determine elections.

A reconciliation between the opposed conceptions of propaganda may not be difficult at the theoretical level. Both schools of thought would probably admit that repetition and intensity of propaganda are only meaningful as they relate to basic needs and existing frames of reference. The conflict is really at the practical level. To reach large numbers of people in a short time, mass communication is necessary. Whether or not mass communication can be geared into personal needs and cognitions, or whether the personal approach can reach enough people to effect social change are practical questions to be tested by empirical research. Some organizations, devoted to a reduction of prejudice against minority groups, have committed themselves

to the personality approach. It remains to be demonstrated, however, that mass communication, skillfully employed, is ineffective in the modification of behavior.

Herz, chief leaflet writer for the Psychological Warfare Division, SHAEF, confirmed peacetime propaganda experience in a recent analysis of the psychological lessons learned from leaflet propaganda in World War II (73). Literal truth may be less plausible than that which is credible from the psychology of the recipient. To tell German soldiers that our prisoners-of-war enjoyed good food with eggs for breakfast merely created disbelief. The doctrine of limited agreement was also confirmed, i.e., the effectiveness of finding some agreement with an existing frame of reference. For example, it was more effective in inducing surrender not to challenge the German soldier's valor but instead to agree with his belief that he was being defeated by superior machines and equipment. The means for evaluating the effectiveness of propaganda included: captured documents on troop morale, leaflets found on prisoners, and interrogation of prisoners-of-war.

Propaganda has generally been regarded as an art rather than a science, but an art which could be helped by operational research. The pretesting of appeals and the research evaluation of the efficacy of campaigns have been moving it toward an empirical science. To push a scientific approach further, Doob has attempted to reduce psychological warfare strategies to a finite number of types on the basis of the possible responses of the target audience (74).

Krugman, in a theoretical analysis of resistance to propaganda, hypothesizes that the person showing the most active resistance to a propaganda message may change the most if resistance is overcome (75). The person who can readily reject the appeal without active resistance is much more difficult to change. As a technique for inducing change, Krugman suggests the presentation of material on mechanisms of resistance, motivation, and rationalization before introduction of the content appeal. The individual may thus develop insight into the usual defenses he musters when encountering a challenge to his established beliefs.

The relation of propagada to personality needs has been explored by Bettelheim & Janowitz in a study of 50 veterans whose tolerance attitudes were previously measured (76). The tolerant group tended to disapprove of anti-Semitic propaganda to which they had been exposed experimentally, while the intolerant group approved. But even some of the intolerant group rejected extreme propaganda and the hypothesis is offered that, to gain acceptance, propaganda materials must diminish existing apprehensions without creating new ones.

The personal insecurity theme appears as the dominant motif in the propaganda of "the small-time peddlers of discontent," according to the analysis of Lowenthal & Guterman (77). The propaganda against minority groups is directed at the little man who has failed to achieve success and who can be made to see himself as the victim of conspirators.

The research findings on the effects of mass media have been summarized

by Klapper and show the need for more precise experimentation with better definition of the relevant variables and more systematic controls (78). For example, on the basis of existing findings it is not possible to give definitive answers to the problems of the comparative effects of the various media of communication.

Some principles of mass persuasion have been formulated by Cartwright on the basis of the studies on the effectiveness of the war bond drives which he directed for the Division of Program Surveys (79). The generalizations are in keeping with the Gestalt laws of cognitive structure but, in addition, Cartwright describes a central factor in mass persuasion that has received very little formal recognition. It is not enough to create a particular cognitive structure and to create a particular motivational structure. It is also essential to create a situation in which the motivational system can gain control over the person's behavior. Although people may favor an action in general, they may do little about it until they are forced, by a situation, to commit themselves. For example, people were impressed by the war bond appeals to their patriotism and their thrift, but the factor that increased the sale of bonds was personal solicitation. This is in agreement with the strategy of the political propagandist, who not only sensitizes people to his particular objectives but who creates an issue on which they must take sides.

Methodology in Opinion and Attitude Research

Though surveys and polls comprise one of the most active and vital areas in social psychology, there has been no comprehensive work covering the techniques and methods until the appearance this spring of Parten's *Surveys, Polls and Samples* (80). The treatment may not be as fundamental as the chapters in the Krech & Crutchfield text, *Theory and Problems of Social Psychology* (81), but it is a highly useful description of the great variety of techniques employed in all kinds of surveys. The history of the specific methods is traced and the contributions of both the commercial polls and the academic researchers are included.

A significant development in survey methodology is its use by the Michigan Survey Research Center in the context of research design. The historical tradition of public opinion polls emphasizes brief interviews with a cross-section sample of the nation, on newsworthy topics. It is difficult to establish relationships between variables on this basis since all measures derive from the same brief interview. The Survey Research Center has been emphasizing programmatic research and the importance of independent measures from more than one source (82).

The survey can be used to give basic findings by the following procedures: (*a*) Obtain independent measures from groups which bear some reciprocal or interacting relationship to one another. For example, in a community leadership study, a sample of leaders can be compared with a sample of followers to give the total pattern of interaction. Or, in an industrial situation it is important to compare the reciprocal perceptions and values of the different levels in the organizational structure, and in addition to measure the

responses of the union stewards and officials as well as the responses of foremen and management (83). Remmers and his students have been making studies of how labor and management view one another compared to their views of themselves (84, 85). (*b*) Obtain independent measures of performance for comparison with interview material on related attitudes, perceptions and values. This is done in polling to some extent in national elections but the comparison here is in such gross terms that it is difficult to get at functional relationships. Industrial organizations with objective measures of performance of groups and individuals provide such a criterion to which the beliefs, values, and attitudes of the members can be related. (*c*) Repeat the same study design in a variety of situations. Survey findings in a single study are suggestive rather than conclusive. It is necessary to repeat the same design in a number of varying situations to determine whether general principles are emerging. The Survey Research Center is following this pattern in studies of organizational morale. (*d*) Use surveys as the before-and-after measures in field experimentation. The survey becomes an even more potent tool in providing measurement for changes occurring outside the laboratory.

Approaching the same problem of the scientific use of surveys from the theoretical side, Rose has suggested that a number of older propositions in sociological theory have applicability in public opinion research (86). He cites as examples the distinction between a public and a mass, the influence of social role upon attitude, communication blocks between classes, the concept of private and public attitude, and the differential enforcement of group values. If public opinion research took such concepts into account, Rose believes, it could become more sophisticated in its methodology. The suggestion is valid but it needs to be supplemented with another orientation, namely that survey methods can help confirm or reject the commonly accepted propositions in social-science theory and can aid in their further reformulation and elaboration.

The National Opinion Research Center (NORC) project on interviewer effects reported three investigations during recent months. This project has attempted to investigate the many specific aspects of the interview situation to find the relevant variables which mediate bias. In one study Stember & Hyman singled out for experimentation the precoding or classification of responses by the interviewer (87). Responses of every other respondent were recorded verbatim, while in the other 50 per cent of the cases, answers were classified in pre-coded categories by the interviewers. No differences were found on three attitudinal questions between the two procedures but a significant difference did appear on a question directed at level of information. The field classification credited fewer respondents with information on the topic than did the home office coding of the verbatim responses. Interviewer expectations and interviewer ideology did not differentially affect field classification among experienced interviewers.

Another NORC experiment examined the effects of interviewer ideology as mediated by question form (88). One half of the sample was given the choice of two alternative answers, while the matched half was provided a

third or middle group category in addition. Interviewers of contrasting viewpoints brought back the same distribution of responses where the middle alternative was included, but there were significant differences between interviewers using the two-alternative form. The interviewers supporting the majority opinion inflated the answers in the category they favored, whereas the minority-minded interviewers brought back an inflated "don't know" response. The distortions tended to occur more for respondents who were moderately involved in the issues than for people heavily involved or people showing complete lack of interest.

In this same program of research Sheatsley found that interviewers did not distort answers on a filter question to avoid asking a series of dependent questions (89). Even interviewers who reported the question as difficult showed no bias in answers which would have saved them the task of asking the dependent questions. There was a bias, however, in that interviewers tended to bring back more answers in accord with their own convictions.

The possibilities and problems in group interviewing have been discussed by Abrams who has experimented with this method with stenographic recordings of the group discussion and a secret vote on a few selected topics after the session (90). The suggested advantages are that the group situation is more like the natural situation than the conventional interview set-up, the discussion gives people an opportunity to voice their real feelings since they think by talking, the discussion sets the frame of reference for evaluating answers in a way that is not possible in the two-person situation, and the group method is more economical.

The disadvantages relate largely to the lack of control in the method. A standardized interview may not in itself be a good basis for prediction to social situations, but it is possible to interpret the data obtained in this manner and relate it to other measurements. Material from an uncontrolled group interview is difficult to interpret. The group interview, if properly controlled, would become very much like the usual small group experiment of the laboratory. Perhaps the best use that can be made of the group interview in public opinion research is as a pilot study for obtaining hypotheses for more rigorous testing in standardized situations.

Projective methods have received some consideration in survey procedures but their possibilities are far from being exploited. Haire, in a study of the morale of retail store employees, found no striking differences between projective and nonprojective methods, though the projective material gave a better feeling for the repressed hostility of the clerk toward customers (91). Ehle has employed pictures and sentence-completion tests in getting at the acceptance of different leadership roles and types. Careful pretesting produced projective instruments that could be used in the field (92).

The unknown selective biases in quota sampling are leading many research organizations to experimentation with probability sampling. The major difficulties with this more precise technique relate to the mortality rate and to the fact that most interviewing staffs lack the personnel and training for the successful execution of precision sampling. Williams illustrates

the problems with figures from the Elmira study in which there was a total mortality of 19 per cent after an initial call and four call-backs (93). Moreover, the diminishing returns with additional call-backs showed little hope of substantially reducing mortality with further visits. On the other hand, Heneman & Paterson report St. Paul studies in which an original mortality rate of 17.6 was cut to 1.3 per cent (94). This was achieved by recruiting new interviewers, training then thoroughly, and supervising them carefully. Even if this phenomenal success can not always be achieved, the essential argument for probability sampling is that the missing cases do not represent unknown holes in the sample. The characteristics of the household can be ascertained and additional efforts can be spent on the people not at home. In quota sampling, the selective bias leaves gaps which cannot be measured and which permit no correction through additional field work. Moreover, Sheatsley has indicated that the refusal rate alone in quota sampling can be as high as 18 per cent (95). Wilson finds that probability sampling is feasible in western Europe because of available lists of people and of households and because of the cooperation of respondents (96).

The neglected field of the validation of responses in public opinion surveys has been brought into bold relief by the work of Parry & Crossley (97). These investigators compared responses of a Denver sample to factual questions with the actual facts. They found that people consistently exaggerated their registration and voting behavior. The exaggeration varied from thirteen per cent who falsely claimed to have voted in the 1948 election to twenty-eight per cent who made fictitious claims to voting in local elections. One third of those who reported contributing to the Community Chest were speaking of pious intentions, not actual contributions. Telephone and home ownership were accurately reported. Similarly, car ownership was not appreciably inflated, but 10 per cent of these reporting a drivers license did not have one. Again, 10 per cent claimed to have library cards when in fact they had none. Where there is a socially acceptable answer to a factual question, the responses in opinion polls are inflated in the expected direction.

A Note on Systematic Texts

In older fields of science, systematic texts serve only a pedagogical purpose. In social psychology, the professional field itself may be affected by a new systematic presentation or the formulation of a new theoretical position appearing in textbook form. Thus Krech & Crutchfield in their 1948 *Theory and Problems of Social Psychology* gave added impetus to experimental studies of perception (81). Their emphasis upon the psychological field and their consistent treatment of all problems in social psychology from the point of view of principles of perceptual and cognitive organization has helped to preserve a hard core of individual psychology as basic to an understanding of social processes. Within the past year, however, two texts have appeared which again raise the question of the importance of integrating social science contributions with psychological observations within a broader framework than that of perception psychology. Sargent has given the field a scholarly

summary of research and theory with more than lip service to sociological and anthropological contributions (98). Newcomb, in his recent volume, has gone further in integrating relevant material for social science with psychological materials (99). The Newcomb treatment bridges the gap between psychology and sociology through concepts of role, reference group, and group membership. Thus, Newcomb provides a framework for the interrelating of existing psychological and sociological concepts. The sociological concept of role, as the expected pattern of behavior of an individual occupying a certain position, is used extensively but it is buttressed by the notion of commonly perceived and shared group norms. In other words, there must be agreement in the psychological fields of the group members about the norms associated with roles. For example, "it is the system of positions and associated roles recognized by group members which provide the shared frames of reference in terms of which people communicate with one another." Moreover, "each group member relates *himself* to the group in a way that he does not relate himself to any other common object" because he must perceive himself and the standards of his group independently. "It is such shared attitudes toward the group as a common value which provide the cement that holds groups together." The advantage of this position is that it enables the social psychologist to go beyond the older, individualistic psychology and explore the complexities of group processes without abandoning a psychological approach to the problems involved.

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INDUSTRIAL PSYCHOLOGY[1]

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Industrial psychologists have published during the past year an amazing array of studies. This vigor has been directed toward the same problems as in previous years with a noticeable increase in emphasis on leadership studies.

There is reason to believe that industrial psychologists are gaining the attention of business and industrial managers. A survey by Brusk & Clark (1) obtained 1,600 questionnaire returns relating to the reading habits of business executives. These men mentioned 237 specific titles of business books. The area of industrial and personnel management was top on the list with 32 mentions, economics was second with 25, marketing was third with 19, labor was sixth with 11 mentions, and all of the rest had fewer than 10 mentions.

It is significant that business executives are reading in areas—personnel management, labor, and marketing—in which industrial psychologists are working. It is unfortunate, however, that many of the published products of industrial psychologists are presented in such a way as to be of little interest and meaning to business executives. Many such psychological publications tend to be highly technical, replete with the verbiage of the academy. The journal *Personnel Psychology*, now in its third year, and the publication *Industrial Training Abstracts*, now in its fourth year, are attempts to bridge the gap between the sound, highly technical, but often unreadable reports of the psychologist and the reading needs of the business executive.

In the line with efforts to meet the needs of business, Dudycha (2) includes in his 115 item bibliography 15 items on careers in personnel work in business and industry. He refers to studies on job specifications, training qualifications, duties, colleges offering courses in personnel, and ethical problems.

Items of General Interest

Baas (3) used the Kuder Preference Record to compare preferences of members of the Division of Industrial and Business Psychology, American Psychological Association, with the preferences of the members of three other divisions. All four divisions seemed to reflect scientific and literary interests. Industrial psychologists tended to have significantly higher scores on the mechanical scale than members of the division of counseling and guidance. Industrial psychologists also tended to have significantly higher scores on the persuasive scale than clinical psychologists.

[1] This review covers the period from August 1949 to June 1950. Acknowledgement is made for the technical assistance of Carol S. Bellows, Alyce Carlind, and Mardell Kroll.

The much used psychologist scoring key for the Strong Vocational Interest Blank was developed in 1938. Do we need a new key? Do we need keys for subgroups of psychologists? Kriedt (4) answers "yes" to these questions. His answers are based on adequate evidence. He (and Strong) recommend use of the new 1948 key. Keys for four subgroups are available, including one for industrial psychologists.

That industrial psychologists are at work on ethical problems and consulting standards is clear from Seashore's report (5) of a symposium of the Division of Industrial and Business Psychology. The problems identified were: methods for appraisal and control of consulting firms; definition of "good taste" or ethical conduct in promoting services; scientific integrity and soundness of methodology; definition and specific instances of questionable or unethical practices; use of confidential materials; and ethical contractual and business arrangements.

Shartle (6) summarized the literature in industrial psychology from April 1948 to July 1949, citing 128 items. A book edited by Fryer & Henry (7) may, several decades hence, be regarded as one of the great historical documents of applied psychology. Of its 115 separate articles (grouped into 18 chapters) a number are cited in various parts of this review. A few of these contain highly original work; most of them seem important as analyses of present trends.

It is unfortunate that more space cannot be devoted here to this Fryer-Henry work. In it, for example, Katzell (8) presents a comprehensive survey of work on and the status of fatigue and its alleviation; Dunlap (9) gives some striking examples of significant savings effected by biomechanics (the application of science to the articulation of man and machines) in military and industrial situations; and Sisson (10) presents a brief but comprehensive statement of the use of personnel research in management of Army and Navy personnel.

Personnel research is being accomplished by psychologists in military establishments at an increasing rate. Agencies responsible for this research include: Personnel Research Section, AGO[2] of the Army Department; Human Resources Research Laboratories of U. S. Air Forces; Human Resources Research Center of the Training Command, U. S. Air Forces; Office of Naval Research and Bureau of Personnel, U. S. Navy. Some of the research is being conducted within these and other agencies; much is being performed by contract with firms of industrial psychologists and with universities. The separate quantitative research reports written as a result of these programs during the last year number close to 100. A considerable portion of these are applicable, especially in methodology used, to business and industrial problems. Unfortunately, published reports of these studies are not available.

In 1949 a conference on current trends in industrial psychology was held at the University of Pittsburgh. A book on trends in industrial psychology,

[2] Adjutant General's Office.

edited by Dennis (11) resulted from the conference. It includes a chapter by Shartle on the Ohio State University leadership studies, one by Flanagan on the use of critical incidents technique for personnel evaluation and one by Crissey on personnel selection. In a chapter on training, McGehee makes a plea for more research, presenting reasons why research may result in considerable economy in training. Katz reviews the work of the University of Michigan Survey Research Center. H. C. Taylor emphasizes the potential for industrial psychology in problems of public understanding, reduction of intergroup tension, and industrial peace.

A number of important problems for the psychologist concerned with industrial relations come under scrutiny in a work edited by Kornhauser (12). The problem of management- versus union-sponsored personnel research is discussed. The need for better communication between psychologists and administrators is stressed; concern is expressed over the failure of both industrial executives and union leaders to make the fullest use of the fruits of personnel research.

Hepner's (13) second revision of *Psychology Applied to Life and Work* contains nine up-to-date chapters on industrial psychology and four on advertising (including a chapter on measuring the effectiveness of advertising) and consumer research. While the book is oriented toward the student, it also should be quite valuable to managers of business and industry. A unique portion of the book gives the student (or on-the-job personnel trainee) guidance in how to read a technical psychological study or research report.

The Interview

Very few studies of interview validity are to be found in the literature. The relation of interview results to verified work histories of interviewees was investigated by Keating, Paterson & Stone (14). The interviews from which they drew their data were more like counseling situations than employment situations. Perhaps for this reason they found high agreement between interviewees' statements and verified reports. For example, they obtained a correlation of .90 between reported weekly wages and verified weekly wages for 115 cases, and higher correlations for reported and verified duration of jobs held.

Moore (15) presents an interesting condensation of his and Bingham's earlier work on interviewing. Chervenik (16) discusses training interviewees for the employment interview and gives a verbatim transcription of a sample structured interview.

The group interview or leaderless group discussion technique is emerging as a promising research area. Bass (17) treats the group interview as a unique technique to aid in the selection of potential leaders. He calls his group interview a "group oral performance test." In this technique, prospective managerial employees are observed and rated in a leaderless group discussion. The data obtained thus far tend to be consistently reliable. The amount of participation by interviewees in this group situation provides a good index of leadership status. The technique seems to offer promise of a more un-

biased measure of tact, initiative, and social skills than do other selection devices.

Bass (18) also summarizes German, British, Australian, and American contributions and discusses the technique, quantitative analysis, and results of his original studies. He obtained fairly high reliability for both sales and supervisory trainee interviews. Subjective and objective measures of amount of participation of "interviewees" in the group interviews correlated .77 and .65, respectively, with observers' selection of candidates.

Bass (19), in another report, presented results on two experimental groups engaged in a series of six structured, leaderless group discussions. Observers rated each participant on a series of 13 items designed to measure leadership status. These ratings yielded a reliability correlation coefficient of .72. A record was made of the time each participant spent in talking at each session. The correlations between time spent talking and the 13 leadership status items ranged from .82 to .92 and the 13 items combined yielded a correlation of .93.

While criterion development and studies of validity are needed, Bass seems justified in his enthusiasm for the leaderless group method as an important research device. It has face validity and sufficient internal statistical consistency to warrant the emphasis Bass gives it.

Selection and Placement Tests

Uhrbrock (20) has been conducting a valuable piece of personnel research. In 1932, a battery of 39 tests (4,738 items in 36 of the tests) was administered to 96 college trained men working in technical positions in the manufacturing department of the Procter and Gamble Company. The study is distinguished by its long follow-up, the detailed care and adequate design on the criterion side, the volume of item analysis work, and the comprehensiveness and clarity of the report.

Uhrbrock sought to determine the effectiveness of each separate item in the trial battery by appropriate item analysis procedure. The long trial battery, requiring some 30 hr. to administer to each man in sessions of 2 hr., consisted of four tests of general intelligence, ten mathematics tests, four chemistry tests, four physics tests, eight English aptitude tests, a vocabulary test, four paper mechanical aptitude tests, a scientific aptitude test, two personality tests, and two interest analysis tests.

Thirty-five executives in the manufacturing organization rated the subjects using order-of-merit rankings, graphic scales, and check lists. The method of developing the check list criteria is noteworthy. A list of 497 statements, e.g., "has inadequate knowledge of the details of the present job," "submits clear, understandable reports," "is uncooperative," was prepared. Ten judges then sorted the statements into 11 piles according to the degree of success they felt would be attained by a person about whom such a statement would be made. From these results a check list of 51 statements, equally spaced from least favorable to most favorable, was derived. Analysis

of the reliability of the composite criterion consisting of ranks, graphic scales, and check list yielded an R of .88.

The Stanford Scientific Aptitude Test for high school and college came through with a zero order validity coefficient of correlation of .46 (64 cases). A Wherry shrunken R of .56 was obtained on 64 cases, with three combined tests. The order in which these tests entered the battery and the accumulated increments of forecasting efficiency were: Stanford Scientific Aptitude Test .46; Columbia Research Bureau Chemistry Test .52; and Ohio State University Psychological Test, Form 17 (work limit) .56.

That tests purchased and used without item analysis contain much "dead wood" is verified by Uhrbrock's item analysis results. Upper and lower 27 per cent criterion groups were used in comparing item responses. In a special study of the OSU Psychological Test, Form 17, two methods of item analysis criterion splits were used: upper and lower 27 per cent; and all cases. The two methods correlated .96. Both validity and difficulty indices were obtained for 4,738 items; and 100 highly valid items, not including personality, interest, and "scientific aptitude," were identified. Uhrbrock's report lists the tests from which test items were drawn. The resulting 100-item omnibus test yielded a validity coefficient of .48 when administered on a work limit basis to 51 men of the original group of 96 subjects. Industrial psychology needs more studies like this one by Uhrbrock.

Wherry (21) presents an important discussion of procedures in item-test analysis, item-criterion analysis and combined test-criterion item analysis. He emphasizes cross validation in item-criterion analysis studies, citing the Biographical Information Blank work of the Personnel Research Section AGO as exemplary. In a lucid section, Wherry gives cogent reasons for analysis of test item alternates—a procedure especially important in personality, interest, and rating scale item work.

Toops (22) makes a plea for research to discover a number of measurable factors "running through the several thousand human occupations," and ultimately to designate occupations by profile numbers and to provide methods for identification of applicants with matched profiles. Dvorak (23) discusses worker analysis techniques for determining vocational interests, physical capacities, occupational skills, knowledge and aptitudes. She summarizes 15 years of work (begun in 1934) of the Worker Analysis Section, U. S. Employment Service.

A reference book by Dorcus & Jones (24) gives these five items of information about more than 400 test studies: tests, subjects, criterion, validity, and reliability. This compendium will be of great value to personnel managers, research workers, and students.

Richardson (25) has long been interested in devices for evaluating, in meaningful production units or dollars, the savings effected by a personnel tool. He presents an easy-to-use formula which yields a meaningful index of increment in effectiveness resulting from an improved personnel technique and illustrates its use by an example. Brogden (26) presents a chart showing relations between test validity, percentage of applicants to be selected, and

savings expected from use of selection tests. He, the reviewer believes, is the first to include in a published estimate adjusted savings in terms of cost of the testing program. The need for including cost of testing is emphasized, since it increases markedly as the percentage of applicants rejected increases.

There has been much concern over the fact that applicant responses to personality and interest inventories can be faked. These devices depend to some extent on the set, or motivation, of the examinee. Rothe (27) describes development of a key, similar to the Minnesota Multiphasic Personality Inventory "L" key, for use as an index to objectivity of responses, or faking. Neither his objectivity key nor the 50-item interview-aid-inventory on which he developed it are available for general use by industrial psychologists. However, it is of significance that he obtained different personality scores on subjects making different objectivity scores. He found it possible by use of the objectivity score to correct personality scores for degree of faking. At the same time, the objectivity key may reveal whether the applicant or interviewee is abnormally frank or naïve in the interviewing situation; it may help yield a more complete clinical picture.

Vision as a selection requirement.—Based on measurements of near and far visual acuity of 5,500 employees, with what appears to be an adequate experimental design, McCormick (28) concluded, in part: Visual acuity requirements are general and not absolute; minima for both-eye vision are pertinent to satisfactory performance; worse-eye acuity is related to job success.

Farnsworth (29) presents an authoritative treatment of color vision. He asserts that some jobs require color perception and discusses its measurement.

Selection of pilot and aircrew.—Flanagan (30) presents recent quantitative results of the effectiveness of tests and other devices as aids in the selection of pilots and aircrew, and shows that reasonably valid objective scales to measure instrument flying skill can be developed. Feronte (31), surveying use of tests for selection and promotion by 24 civilian air transport companies, found that 13 used tests extensively, two used tests to select pilots, one planned to use tests and six were small companies with employment volume too low to justify their use.

Selection of magazine editors.—Parts of the Michigan Vocabulary Profile Test seem to discriminate between high and low criterion groups of magazine editors as reported by Abt (32); several other test variates were found to be statistically discriminative, but the Bernreuter Personality Inventory scores were not.

Selection of office personnel.—Giese (33) used the follow-up method of checking the usefulness of psychological tests in the selection of office personnel over a two-year period. He demonstrated that most of the employees who were eventually discharged for unsatisfactory work would have been eliminated in the employment office on the basis of their performance on the test battery.

Selection of supervisors.—Ratings were correlated by Holmes (34) with measures of ability, achievement, and personality for 269 supervisory, "interpretative" and skilled employees in an insurance office. Criteria consisted of two periodic ratings, several raters participating, but no reliabilities of criteria were included (this seems a weakness of the study). Multiple *R*'s of around .60 were obtained for all three groups. The "How Supervise?" test seemed valid for the measure of supervisory success for one group but did not for another. Knauft (35) attempted to predict managerial success in a bakery company. He employed a test battery consisting of the Baking Knowledge Test, Jurgensen's Classification Inventory, a management test, and a mental ability test. Test scores were compared with a composite criterion score on 79 retail-manufacturing bakery chain managers which were used as a criterion group. In a follow-up study of 33 newly hired managers, only the Classification Inventory yielded a validity coefficient significant at the 5 per cent level, although the other tests appeared useful for distinguishing between satisfactory and unsatisfactory managers at the extremes and for determining training needed.

Lawshe (36) reports a study in which 176 supervisors from 44 widely scattered industries (two considered "best" and two "poorest" from each plant) were given The Adaptability Test. He found that 63 per cent of those who achieved a score of 12 or better actually were in the "best" group. He concludes that this 20 item test measures "one of the common denominators of supervisory success."

Selection of sales personnel.—Eimicke (37) presents norms for 471 sales trainees on several measures including the Kuder Preference Record. These trainees scored lower than "the general population" on mechanical, computational, and scientific interests; higher in literary, musical, and social service interests; markedly higher in persuasive interests.

The Life Insurance Agency Management Association has shown continuing interest in predictors of success in life insurance sales. Wallace & Twichell (38) found, in a study of over 2,000 U. S. and Canadian agents, that survival in the company and success were related to selection test scores. The Aptitude Index was used in selecting many of the men in the study. Other factors besides test scores, however, were found to operate in determining success or survival. Two of these were: the financial basis on which the men were contracted and their stated financial needs.

Harrell (39) compared the success of package salesmen with scores on the Humm-Wadsworth Temperament Scale. He found no statistically significant difference in the Humm-Wadsworth components for the successful and the unsuccessful salesmen. It is possible that the Humm-Wadsworth scale may be employed by a "specially trained clinician" to yield somewhat better than chance prediction of success. In the reviewer's opinion, quantitative evidence for this allegation is not yet available.

Bills (40), apparently referring to her 1938 study, says that the Strong Vocational Interest Blank is effective in selecting insurance salesmen: "a

man scoring high has three times the chance of success that a man has scoring low." It seems fairly clear from her report that the Strong key for insurance salesmen was used, but "scoring high" and "scoring low" is not defined.

Selection of mechanical personnel.—Ammons (41) has undertaken development of a publication that gives information on current research studies in motor skills. He started it with Vol. 1, No. 1, in March, 1949, producing in that year four mimeographed issues totaling 36 pages. It is quantitatively oriented.

The problem of the older worker has come increasingly to the fore as the average age of the general population has increased. Brown & Ghiselli (42) administered 10 tests and an interest inventory to several hundred semiskilled workers. Results obtained indicated that the older workers were as proficient as the younger ones in speed tasks which do not call for precision or complex mental processes and in tasks involving familiar materials and operations. No significant change in interests was found except that the older workers preferred jobs at a somewhat higher occupational level.

Supervisory ratings were found by Kerr & Martin (43) to have a split-half reliability coefficient of over .75 for 244 shop employees. These ratings were compared with the 11 items of greatest significance on the application blank. The combined scores of these 11 items produced a correlation coefficient of $.35 \pm .04$ with the supervisory ratings. Items of this study having the greatest relation to the criterion were marital status, area of residence, membership in organizations, and special education. The autobiographical information obtained from the application form sometimes offers a valuable contribution to the total predictive test battery but can be used effectively only when validated and applied to selection for specific job classifications rather than a hiring standard for the entire organization.

Selection of skilled workers.—Criteria consisting of two supervisors' ratings (reliability of .87) of power sewing machine operators were correlated by Glantz (44) with a worksample trade test to determine the test's forecasting efficiency. Parts of the trade test correlated from .55 to .64 with supervisors' ratings and from .31 to .56 with production records.

A new test of mechanical comprehension has been developed and partially evaluated by Owens (45), who points out that Bennett's Test of Mechanical Comprehension is widely used (e.g., it has been reported in use in 90 per cent of Veterans' Administration Guidance Centers and in frequent use in 64 per cent of them). Owens' new test is alleged to be more difficult than Bennett's, but no evidence of this is included in his report. He does include reliability (.80 adjusted) and validity in predicting various criteria of success in engineering school. Owens' introductory remarks and his discussion of results strongly imply that the new test is more useful than the Bennett test. It is unfortunate that Owens did not obtain Bennett test data on the same population so that direct comparison of forecasting efficiency could be made. This comparison is urged for the very reason that the Bennett test has been widely used. Comprehensive norms and validity information on samples are available from military, educational, and industrial situations.

Twenty characteristics from the United States Employment Service Occupational Characteristics Check List, together with two other traits, were reduced by factor analysis to six meaningful factors: strength, intelligence, inspection, physically unpleasant working conditions, manual dexterity, and mechanical information. This was reported in an interesting study by Jaspen (46), who suggests greater exploratory use of strength tests in selection.

A time and motion analysis of four MacQuarrie Mechanical Ability subtests was made by Goodman (47). The time and motion analysis revealed a visual inspection factor and a controlled manual movement factor; factor analysis of the resulting data identified a space factor and a manual movement factor.

Harrell (48), who bases his conclusion on a considerable amount of quantitative evidence, believes that mechanical tests include one or two dexterity factors, a spatial visualization factor, possibly a perceptual factor, and a space relations factor. His review of validity data obtained in selected studies is a valuable one.

Anderson (49) found that the group Rorschach method was not particularly effective in differentiating adult male machinists with high efficiency ratings from those receiving low ratings. Combined scores on the Adult Placement Test and the Bennett Mechanical Comprehension Test discriminated better between the two groups.

Skilled trades (vocational interest).—The 190 triads of items on the Minnesota Vocational Interest Inventory were completed by over 1,100 members of eight American Federation of Labor trade unions in a study by Clark (50). He describes the procedure for scoring and establishing the eight occupational keys. His results indicate a clustering of trades with respect to measured interest. The electricians, plumbers, and sheet metal workers have related interests but differ markedly from bakers, milk wagon drivers, painters, and plasterers.

Worker characteristics and safety.—Accidents cost 6.4 billion dollars in 1946, of which 2.4 billion were lost in industrial accidents. Teal (51), who surveys research in this area, maintains that improved selection, training, supervision and records, together with their analysis and statistical interpretation, will reduce this tremendous loss. The improvements must come in large part from personnel research. Forbes (52) reviews the more recent and better controlled studies in which psychology has helped solve highway traffic problems with special emphasis on traffic safety.

Tiffin, Parker & Habersat (53) found that professional eye attention re-resulted in a definite decrease in injuries and that visual safety standards seemed to be associated with a marked decrease in accidents and compensation costs. Whitlock & Crannell (54) analyzed 100 accidents in a steel plant, finding that neither nontest data, mechanical comprehension, nor Otis scores differentiated accident groups from control groups; neurotic, introvert, and self-confidence scales of the Bernreuter Personality Inventory "seemed to differentiate to some degree."

A study of accident-prone employees at Benicia Arsenal substantiates the results of many earlier investigations. Holmes (55) studied 105 accident-prone workers (four or more accidents) out of a total of 8,138 employees. Aptitude as measured was found to be negatively correlated (−.21) with rate of accidents. More employees between the ages of 40 and 48 were injured on the job than was the case for a younger group; but accident-prone employees who were 30 years old were injured more often.

Test scores of 67 taxicab drivers on eight paper and pencil tests and an interest inventory, together with personal data items, were correlated with accidents during the first five weeks of employment by Ghiselli & Brown (56). Dotting and tapping tests yielded the highest validity coefficients of .35 and .47, respectively. A weighted combination of dotting, tapping, and three other tests yielded a validity coefficient of .59. The arithmetic test and the personal data items produced no significant relationships with the criteria, although interest measures showed some promise. Safety is a fruitful, although complex and difficult problem, a challenging field for young applied psychologists who are choosing an area for research contribution.

Job Analysis and Job Evaluation

Shartle (57) tells how to obtain job information, explains its use and discusses job classification and evaluation. He holds that fewer job factors can do the work of many in job evaluation.

Flanagan (58) discusses the possibility of an approach to employee evaluation through the establishment of critical requirements for different jobs. These critical requirements can only be established by the careful study of the behavior of workers on the job. i.e., by job analysis. A critical requirement is defined as "a requirement which is crucial in the sense that it has been responsible for outstandingly effective or definitely unsatisfactory performance of an important part of the job or activity in question."

In a study by Stewart (59), four companies conducted an experiment to determine the comparability of apparently similar jobs as evaluated by each company. Twenty-one jobs were selected, all covered by master definitions. They were evaluated under the plan of the National Metal Trades Association by four persons acquainted with the plan. Each person evaluated the jobs as he saw them in his own company. The study reveals that one of the evaluators had a consistent liberal bias. A study of this sort points out variations that should be investigated. Stewart asserts that statistical methods are valuable to determine the reliability of the evaluators, to coordinate the evaluations at various locations within the company and to measure bias and make adjustments for it. To demonstrate how the weakness of halo effect can be used to increase the reliability of planned job evaluation, a study of the factor of experience in the evaluations was made.

Job analysis and job evaluation were shown by Rush & Bellows (60) to be applicable to a small business of 60 employees with 24 jobs. The two factors used in the job evaluation were (*a*) responsibility and (*b*) training and educa-

tion. The two factors were found to be so highly intercorrelated that it was suggested that either factor alone would have yielded about the same results.

Hay (61) proposed to eliminate the "reduction" operation in the procedure for establishing factor comparison scales by the per cent method of job evaluation. Turner (62) warns against the use of Hay's abbreviated procedure. He includes a discussion of the mathematical relations underlying the per cent method, alleges that Hay is not using Weber's law as Hay claims, but a law of per cent judgment which is psychological but not psychophysical.

Criteria

In a carefully controlled quantitative study, including four factor analyses, Wherry & Fryer (63) found that buddy ratings (rating by peers) have reliability, identify high and low members of a group on "leadership" earlier than some other criteria, have high predictability, and agree with personnel actions based on other measures.

The forced-choice technique was developed by the Personnel Research Section AGO in 1945. In a discussion of personality tests and prediction, Rundquist (64) presents one of the clearest of several available statements of development and evaluation of this technique together with a list of seven of its advantages. The reviewer believes that it has several disadvantages in use (if not combined with conventional operational criteria), the main one being that it does not lend itself to counseling and personnel development. The forced-choice method is a personnel evaluation device only. Rundquist's treatment of the development of the Biographical Information Blank is comprehensive.

Wherry (65) stresses that criteria are essential to evaluation of training, selection, suggestion systems, medical examinations, and rating scales. Rating scales used as operational criteria may be validated against "ultimate" criteria—an important concept. He devotes considerable space to sources of criterion contamination and ways of checking the goodness of criteria. He discusses the Horst, Edgerton & Kolbe, Hotelling, and Wherry methods for combining criterion variates.

Rating, defined as the process of judging people and things, is discussed by Paterson (66); he points out that the forced-choice method as developed by the Personnel Research Section AGO followed prior development of a prototype in personality testing by C. E. Jurgensen. Paterson asserts that the forced-choice procedure results in high reliability and validity and avoids common pitfalls in rating.

Gordon (67) developed a flight check method of high obtained reliability. The flight check comprised tasks judged to be "critical" to the job on the basis of job analyses.

The American Management Association has produced a rather comprehensive manual of merit rating techniques, edited by Dooher & Marquis (68). It includes articles on the rationale of the forced-choice procedure by

Richardson, on the validity of ratings by Tiffin and by Driver, and on the development of systems by Bittner. Emphasis is correctly placed by Martucci on joint development of systems by management and labor. The general usefulness of the forced-choice method of merit rating is described by Richardson. Essentially the job of the rater is easier by this method than by other methods. The rater is asked to report on behavior without being required to evaluate that behavior. A further advantage is that the rater need not concern himself with the question of whether his report is favorable or unfavorable. The need for further research in the forced-choice method of measuring performance is stressed.

Although the forced-choice scale is felt to be the most effective method for evaluative purposes, it is likely to be of little use in assigning work or raising morale. A rating procedure described by Rundquist and Bittner hinges on each rater developing for the jobs with which he is concerned a "job definition," i.e., a list of the important job elements to which he will refer in rating a worker's performance. Rating judgments are then concrete rather than being made on general traits. Supervisors have an opportunity to analyze the real elements of success on the jobs they supervise. The supervisor is able to counsel his subordinates with reference to their strengths and weaknesses in specific terms.

The advantages and disadvantages of several rating systems, the chart method, the rank-order method, and the forced distribution system, are discussed by Tiffin. The forced distribution method with a small number of traits and accompanying check list is recommended as the most reliable and accurate method in most situations.

Ferguson (69) has found in a study for the Metropolitan Life Insurance Company that an appraisal form built on the basis of careful research and applied under controlled conditions tends to minimize difficulty with raters displaying the lenient tendency and raters who usually rate everybody about the same. In a quantitative study Ferguson (70) also found that both criterion reliability and validity are markedly influenced by degree of the rater's acquaintance with ratees.

Training

Pressey (71) summarizes his project study of educational acceleration; this work is of particular interest to industrial psychologists who are concerned with the vastly important area of industrial training.

Managements' use of personnel research in training is considered by Bellows (72). He relates training to other areas of personnel methods, emphasizing communications as a training device. A study is described in which a sample of some 15,000 experimental cases were used; results are presented in terms of savings to management in executive trainee placement and evaluation.

Leadership

Hemphill (73), after surveying the literature on problems in the study of leadership, concluded that leadership must be approached by methods

for analyzing (*a*) leadership behavior, (*b*) the social group situation in which the leader functions, and (*c*) judgments of leadership adequacy. He reviews the development of ways of studying social group dimensions, finding it possible to quantify each of the following 15 dimensions on a continuum: size, viscidity, homogeneity, flexibility, stability, permeability, polarization, autonomy, intimacy, and control pertaining to the group as a unit; and position, participation, potency, hedonic tone and dependence, expressing an individual's relation to the group. The reliability coefficients for these dimensions ranged from .53 to .95.

The leadership adequacy criterion consisted of the sum of two ratings of the person's leadership quality. This made possible the study of the relation of the dimensions of the group to leadership quality. Leadership adequacy correlated with the dimension viscidity .52 and with hedonic tone .51. These were the highest correlations obtained, the other 13 ranging from −.06 to .29.

A 55-item leadership bibliography is included by Hemphill; the appendix presents: contingency coefficients (some 5,000 were computed in the study) expressing relations between group dimensions; leadership adequacy and behavior of leaders; and a facsimile of the final form of the questionnaire for the study of group behavior. Hemphill considers his study, which is also summarized in another place (74), as exploratory and preliminary. It is a pioneering effort that may well establish the foundations for further development and evaluation of methods of study in an extremely important area for the military and industrial psychologist as well as the social psychologist.

Studies are presented in an undated mimeographed document by Hemphill (75) in which the Leadership Behavior Description Questionnaire was administered to over 350 persons, some of whom were leaders and others of whom described leaders of groups in which they were members. The principal question facing the investigators was, "How does a leader carry out his leadership activities?" From an interpretation of the factor analysis of the leader behavior dimensions, three major ways of accomplishing the leadership job emerge: (*a*) a leader may stress being a socially acceptable individual in his interactions with other group members, (*b*) a leader may stress "getting the job done," (*c*) a leader may stress the cooperative aspects of the group activity, i.e., operating in the leader position as a "group catalyst."

Browne (76) developed R, A, and D scales which offer interesting possibilities for measurement of responsibility, authority, and delegation of authority in industrial leadership situations. In applying his scales to a small number of cases, he found that the executives estimated the authority which they delegate to be less than either their own responsibility or authority. They also tended to estimate their authority as less than their responsibility.

Browne (77) reported another exploratory study of executive leadership. In this study he developed and analyzed a method for studying goal and achievement. His method for obtaining the goal and achievement index amounts to asking different executives: (*a*) what things they are trying to

achieve and (*b*) how well they are achieving them. He found it possible to compare goal with achievement by echelons of executives and by departments. The results, based on 23 cases, are suggestive as to method for (*a*) appraising communications (e.g., if other executives respond the same to goals and achievements as the president and general manager, then communication is effective), and (*b*) interpreting various aspects of the relation between goal and achievement. His index may well be a prototype for future methods of studying executive leadership in business.

Stogdill (78) defines leadership as influence upon the activities of the organization rather than in terms of influence upon persons. This definition removes leadership from the vague realm of social interaction and associates it with basic variables which describe an organized group. Further, it suggests methods for studying leadership in terms of work performance, work methods, and working relationships.

Remmers & Remmers (79) found in a study of union leaders and industrial managers based on the test "How Supervise?" that (*a*) among labor leaders the younger, less experienced men tend to earn higher scores than the older men, and (*b*) labor leaders tend to stereotype management, believing that management is less understanding of good supervisory attitudes and methods than it actually is.

In a quantitative analysis of data from an Indiana sample, Miller & Remmers (80) reached certain conclusions of interest. Industrial managers tend to believe that labor leaders as a group have a higher level of supervisory knowledge than they actually do; labor leaders tend to stereotype managers in the opposite way. Feelings of hostility probably accompany this stereotype. Management as a group has improved its understanding of the principles of good supervision (as measured by "How Supervise?)."

Employee Morale and Communication

"The real issue wasn't the 15¢ an hour we asked for or the 5¢ we got. The real cause of the strike was that we had to convince that guy he couldn't be a little dictator any longer!" This quotation, given in Stagner (81), is from a union organizer. Stagner uses it to sum up his idea that industrial conflict " . . . is the problem of democratic self-assertion without democratic controls . . . (We must) develop techniques for sharing . . . ego goals." He places emphasis on frustrated egos of workers as more important than earnings; he refers to wage controversy as the "dollar fallacy."

Hoppock has provided six summaries of job satisfaction researches since his 1935 book titled *Job Satisfaction* (82). He and Robinson (83) present the seventh of this series, including a 26-title bibliography. They make a plea for emphasis in selecting and counseling upon "emotional fitness" for the job, claiming that this is as important as aptitude and interest, albeit inconvenient to measure.

Employee publications, according to Raney (84), need to be evaluated in terms of two objectives: (*a*) whether the publication carries topics appropri-

ate to the situation; and (*b*) whether the workers understand what is written. He made an analysis of the content of several company papers and classified the material under five general headings: personal news, plant news, product news, management information, and employee programs. An application of a modified form of the Flesch readability formula indicated that about 65 per cent of the material in an unselected group of company papers was either hard or very hard to read; few employees would be able to read it with adequate comprehension.

Hayes, Jenkins & Walker (85) found that the Flesch readability formulas and the directions for their use were sufficiently objective for use by inexperienced analysts; they also found by analyzing quantitative data that reliability of the reading ease index was satisfactory. Farr (86) stresses the use of the readability formula in preparing management-employee communications.

Giese & Ruter (87) attempted to predict morale of departments from objective data. The morale data were obtained by scoring a questionnaire; the objective data consisted of six department variates including turnover, productive efficiency, and lateness. They obtained a correlation coefficient of .71 between the morale measure and the six variates. These six weighted variates are referred to as an objective index of morale.

A large manufacturing company has conducted an essay writing contest in which nearly 175,000 employees (58.8 per cent of those eligible) participated. Considerable promotional fanfare was used to encourage participation and many prizes (5,145, including 40 autos) were awarded. A number of reports of the contest are available, including four articles by Evans & Laseau (88 to 91).

Specifically, the company listed four objectives of the contest (88): (*a*) to encourage more constructive attitudes; (*b*) "to place certain educational bulletins in the hands of employees that would indicate some of the benefits derived from employment with General Motors"; (*c*) to obtain information for the enlightenment and education of members of supervision and management; and (*d*) to secure data bearing upon employee attitudes.

The contest avowedly "accentuated the positive." The general subject given the participants was "My Job, and Why I Like It." The entry form used in the contest did, however, provide for unfavorable or critical comments. Only about 7 per cent of the participants used this part of the form. It seems clear that "the typical entrant gave attention to what he considered important aspects of his experiences that might influence the judges to consider his entry favorably" (89). It is difficult for the reviewer to reconcile this mental set on the part of the participant with the statements that the technique " . . . produces a state of mind that is open and undirected. . . . When related (compared) to the source material provided by various types of structured, formalized questionnaire and personnel interview approaches, MJC (My Job Contest) appeared to have great advantages in the purity of reflection of the employe's attitudes and opinions" (89).

The studies are controversial in respect to "purity of reflection of the employees' attitudes and opinions," since entrants may certainly be assumed to have had a definite, structured set to discuss favorable aspects of their jobs. No published critical articles have as yet come to the attention of the reviewer; however, there has been some adverse discussion, largely based on inconsistency such as that referred to above, which is not uncommon in the reports cited. The reviewer believes that the contest was sincerely conducted for the four objectives listed.

The main information derived from analysis of the essays was frequency of mention of favorable aspects of the job. A 10 per cent sample was used in the main analysis. Evidence is presented as to adequacy of this sampling. It was possible to determine, on the basis of frequency of mention, which divisions of the company were significantly lower or higher in terms of the adequacy of such facilities and conditions as: cafeteria, employee publications, steady work, medical facilities, opportunity for advancement, supervision, wages, safety, working hours, insurance plans. It was further possible to relate, in the case of some of these items, the indices based on frequency of mention to actual conditions within a plant or division. For example, one division had frequent mention of opportunity for advancement; this division had experienced considerable growth and was expanding its work force. This external consistency seems, in general, to validate the data obtained, and to justify communicating the results to managements of the divisions for their information, indoctrination, and action.

Holdrege (92) obtained important morale data from 80,000 civil service employees in Air Matériel Command. He used a 24 page questionnaire entitled "How I Feel about My Job." The Command plans to use the results in formulating personnel policy. A second survey is planned to measure any improvement.

Music was introduced in a plant where 142 women employees were performing a complex job in a relatively stable work situation. McGehee & Gardner (93) found the music to have negligible effect on production records. A questionnaire given the participating workers revealed, however, that they enjoyed the music and "felt it was beneficial to them in the performance of their work."

Bradshaw & Krugman (94) discuss morale problems found among several types of industrial employee groups and suggest communications devices, especially the multidimensional attitude interview, for engendering worker-management understanding.

Advertising and Market Research

A total of 3.9 billion dollars was spent in advertising in 1947, according to Lucas (95). Corby (96), in presenting copy testing as one of the arts of psychological practice, gives examples of much money saved by its use. One advertiser, who spent more than $75,000, got few returns. A survey was made revealing clearly that the wrong specific appeal was made; the trip to Holly-

wood was unwanted by potential users of the product, who were married women, many of them with children. Moral—survey before, not after, spending the money.

Jenkins (97) wrote a highly significant paper on customer research, based on the crucial thesis that existing buying behavior, what the consumer says and does, is the key to future buying behavior. General lists of human motives are entirely inadequate. He discusses techniques of inquiry and field investigations. The recent application of psychology to radio and television problems is treated by Dichter (98), who shows how research helps to make programs more effective.

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COMPARATIVE AND PHYSIOLOGICAL PSYCHOLOGY[1]

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Comparative psychologists have long needed an orienting theoretical approach. The theory of evolution, of course, has provided such an orientation for systematic biologists and comparative anatomists. Much of the time, however, comparative psychologists pay little attention to the relations between their data and evolutionary theory; nor do they attempt to see relations between their data and other comparative biological information.

This deficiency is accented in Lashley's recent paper (1). He observes that the comparative study of behavior and the comparative study of the nervous system are still largely separate in method, despite a large literature in both fields. Lashley attempts a beginning of the integration of comparative and physiological psychology with other related biological disciplines. He suggests some hypotheses to account for behavioral differences in closely related species. Comparative psychology needs more such evaluation of its ends and methods.

Without an established framework for comparative psychology, the authors of this review cannot synthesize very well the articles that have appeared in this field in the last year. For most of the studies we shall review, the reader must fill in his own background and orientation.

Studies of Sensory Capacities

Comparative studies of sensory capacities were more diligently pursued by psychologists 20 years ago than today. A few studies from the psychological and biological literature, however, are of interest to students of behavior.

In an interesting review, Waterman (2) takes up the mechanisms for controlling the flight of insects. He makes use of analogies with flight instruments in man-made flight, and his analogies are about as plausible as those found in any of the recent, similar attempts in other fields. One analogy is to an air-speed indicator. Hollick's work (3), he points out, shows that the detection of air speed is due to Johnston's organs on the antennae of the fly. Movement of the air at the head of the insect causes a differential air pressure that deflects the third joints of the antennae. Thus is Johnston's organ stimulated, and it is responsible for the reflex adjustments.

A turn indicator, Waterman proposes, is provided in the Diptera by the auxiliary pair of wings which are not functional for flying. At their bases

[1] This review covers approximately the period from June, 1949 to May, 1950.

are masses of sense organs which function as "gyroscopic turn indicators." An analysis of the oscillatory motion of the vestigal wings (halteres) by Waterman (2) shows how the physical properties of their movements can give rise to the necessary sensory input.

Finally, for the existence of an insect polarized-light compass, he draws upon von Frisch's observations (4). Von Frisch showed that it was not necessary for a bee to see the sun in order to maintain its directional orientation based upon light. A small patch of open sky would do. If one uses a mirror to reflect the image of open sky to the bee's eye, the insect will take a new bearing that corresponds to the angular difference of the original and reflected images of the sky. Apparently the partial polarization of light is the factor accounting for the light-induced directional orientation of sky light. This, of course, means we must assume that the animal's optical equipment includes a polarization-analyzer. To support such an assumption, Waterman cites his own unpublished observations that the *Limulus* eye shows differential sensitivity to light polarized in different planes. Waterman points out that we still have little or no idea as to how the bee knows the bearing of the sun from the angle of polarization it sees.

Like the other insects for which we have data, the dronefly, *Eristalis tenax*, is capable of color discrimination (5)—at least well enough to differentiate yellow from grays and other colors. Another study tells us that the blunt-nosed minnow, *Hyborhynchus notatus*, can discriminate the odor of aquatic plants (6). The ability serves in guiding the fish to feeding grounds, and when their olfactory epithelium is destroyed, they can no longer use the plants as guides.

Some psychologists may be interested in the fact that the rat can discriminate time. Heron (7) trained rats to discriminate between a delay of 5 sec. and a delay of 40 sec. in a maze. Some of the animals could distinguish a delay of 35 or of 25 sec. from one of 5 sec. Only one animal could discriminate a delay of 10 sec. from one of 5 sec. Heron offers no experimental tests to show what cues, if any, the rat uses to make the discriminations.

Studies of Learning and Behavioral Capacities

Psychologists have obtained conditioned responses in a great many living things. Hunt (8) now adds to our list a conditioned response in the chick embryo of 15 days incubation. Once set up, the response is "remembered" until hatching. A little later, however, it disappears.

From Seidman (9) comes a report of a truly comparative study of the relative ability of the newt and terrapin to reverse a direction-habit. The terrapin was superior on all counts. This greater "plasticity," Seidman suggests, may be due to a true nonolfactory cortex, which is present in the terrapin but not in the newt. More such comparative studies of species, which differ in some important biological respect, might take us a long way in understanding the evolution of behavior. We hope that there will be more of this kind in the future.

Mating behavior was the subject of a few experimental and review papers

in the last year. In Hoch & Zubin's *Psychosexual Development in Health and Disease*, Beach (10) has a thoughtful and scholarly review of the comparative study of mating behavior. Accompanying Beach's article is one by Gantt (11) on psychosexuality in animals. This article is largely a summary of his own observations upon experimental animals. In addition, Schlosberg *et al.* (12) report the elaborate mating behavior of two species of viviparous fish, and Aronson (13) describes something that looks like spontaneous emission in the cat.

Searle (14) published an interesting and important study of hereditary maze brightness and maze dullness which was reviewed by Thorndike in Volume I of the *Annual Review of Psychology* under the topic of INDIVIDUAL DIFFERENCES. Several studies show that there are opportunities for improving comparative methods. The analysis of variance, Meyer (15) points out, has some useful applications in a multivariate analysis of behavior. Nissen *et al.* (16) indicate that some of the troubles in making comparative studies come about because it is easy to confuse such factors as attention, etc., with capacities. As an example, they cite how hard it is to induce chimpanzees to learn conditional matching behavior, probably because the chimpanzee cannot concentrate on just a few aspects of the situation. Hebb (17) suggests ways of studying individual differences and anthropoid temperament so as to compare it with man's.

Finally, under the heading of comparative studies, we may report several studies dealing with social factors in the control of animal behavior. Calhoun (18) gives good evidence that social conditioning has much to do with controlling population growth. He draws on Mirone's observation (19) that poor diet in the mother may make for high litter mortality in mice. Then he shows how, in rats, the relative location of the nest and food supply socially conditions animals in ways that result in poor maternal diet. Two other studies on social control of behavior in mammals show: (*a*) that age is a significant factor in the dominance patterns of goats (20), and (*b*) that social facilitation is a factor in the feeding behavior of dogs (21).

Studies on the Nervous System and Behavior

Studies on localization.—In recent years we have witnessed important developments in techniques of electrically recording the activity of the central nervous system induced by afferent stimulation [see Woolsey (22)]. These developments greatly broaden the possibilities for studying the relationship between behavior and different parts of the central nervous system. Type and amount of anesthesia to use with such techniques, however, have posed important difficulties. Jarcho's recent study (23) of the excitability of the cortical afferent systems under a barbiturate anesthesia may help overcome some of these difficulties. He specifies the optimal anesthetic conditions for electrical mapping of the cortex.

Two recent papers describe brain stem electrical responses to auditory stimuli. In one, Thurlow & Gross (24) made microelectrode recordings from the inferior colliculus of the cat. They found responses for steady tones,

frequency "sweeps," onset of steady tones, clicks, and thermal noise. Sometimes, they could obtain responses from the inferior colliculus to changing frequency (the "sweeps") when none of the separate frequencies through which the change occurred were themselves effective. The report is too brief to evaluate adequately, but it suggests that certain cells at least respond only to noise and so require summation in order to respond. Thurlow & Gross also describe frequency-specific responses throughout the auditory range [cf. "response areas" of Galambos & Davis (25)]. Their results, they suggest, provide valuable supporting evidence concerning the reception of low tones. Galambos & Rosenblith (26) have likewise recorded electrical activity to auditory stimuli from the inferior colliculus as well as from the cochlear nuclei. The responses are of two types: (*a*) a slow wave and (*b*) a spike discharge. The slow wave is the response which has been used in the past in tracing pathways, localization studies, and inferring the properties of pitch, loudness, etc., in the central nervous system. The spike is a discharge of cell bodies oriented with respect to the microelectrode tip. Latencies of the spike discharge are longer than those of the slow wave and they so increase as one goes up the tract that the slow wave higher up may appear before the spike does lower down in the pathway.

Also, making use of electrical techniques, Patton & Amassian (27) have reopened the question of the cortical receptive zone for taste in cats. From other ablation work, it would seem that the "taste area" lies in the somatosensory face area, but this is not what Patton & Amassian find. They stimulated the chorda tympani nerve, which mainly carries taste impulses, although it includes tactile fibers too. While stimulating in this way, they mapped the area of cortical response. This was found to be on the rostral orbital surface, superior to the rhinal fissure and cephalad to the anterior ectosylvian fissure. It was rostral and inferior to the tactile zone of the face and overlapped this region in its dorsocaudal extent.

Similar studies of the cortical projection of proprioception in cats and monkeys are described by Gay & Gellhorn (28). The chief cerebral area responsive in the cat was the sensorimotor cortex of the contralateral side. In the monkey, the major effect was within the precentral motor region (areas 4 and 6). In some of the cats, the response was more diffuse than in others, and, in general, the response was more diffuse in cats than in monkeys. This, however, could have been due to uncontrolled variations in amount and effectiveness of the anesthetic agent, dialurethane.

Casting doubt on the generally accepted map of areas of the motor cortex of the monkey is a study by Woolsey & Settlage using electrical stimulation. "The motor pattern of this animal also hangs together as it does in the rat. . . . The accepted separation of face and trunk is not a fact" [(29) p. 140]. The authors describe the pattern of representation in some detail and question whether the usual separation of the precentral region into physiologically distinct motor and premotor fields is justified.

The discoveries made in recent years by physiological mapping techniques should reopen many inquiries about the validity of certain results of ablation

studies. The data of many ablation studies of the past are now obsolete hence the studies are in need of repetition. Indeed, a program of research involving joint use of electrical mapping, ablation, and behavioral techniques ought to give valuable and perhaps very exciting new conclusions about cortical function.

An important paper by Glees & Cole (30) presents direct evidence for a limited kind of equipotentiality. Taking a cue from some early observations of Grünbaum & Sherrington (31), the authors made small cortical lesions in the thumb or hand region (in area 4) of monkeys. Immediately afterward, these animals could not perform a delicate and skilled motor act (opening a small puzzle box), but they soon recovered from paralysis and regained their manual skill. Before the investigators concluded the experiment, they again exposed area 4, stimulated it, and reoperated. Parts adjacent to the lesions in the hand area gave hand responses which had not been detectable before the lesions were made. Then, undercutting these regions, they again produced paralysis and loss of motor skill in the previously recovered arm. In two of the animals, several more operations were performed. In general, the same results held even to the point of almost complete uni- or bilateral ablation of area 4. However, the authors seemed to be of the opinion that delicate, skilled acts disappeared when the region was entirely missing. At any rate, they suggest that intact parts of the motor cortex can develop a latent ability to control the function of previously removed areas.

Unfortunately, Glees & Cole (30) do not include a very careful description of the electrical stimulation techniques used. Lacking that, we can entertain several interpretations of their results. The entire area stimulated may have the ability to control hand movements, but the electrical threshold for movement may change after operation. Or the new areas may acquire functional control, as the authors suggest. Or, finally, the observations on electrical thresholds may simply represent uncontrolled variations in stimulation and/or thresholds.

Also demanding brief attention are many other studies of localization of function and of functional relationships within the central nervous system. Kellogg (32) shows that circular walking in dogs, characteristic of hemidecorticated animals, is definitely not a type of visual defect but is basically motor in origin. Pool & Ransokoff (33) report on the autonomic effects of stimulation of the rostral portion of the cingulate gyri in man. They find that bilateral stimulation of this region alters blood pressure, pulse, and respiration. Kaada, Pribram & Epstein (34) also observed respiratory and vascular changes when they stimulated the temporal pole, insula, orbital surface, and cingulate gyrus of monkeys. Borison & Wang (35) continue to work on the functional localization of vomiting in cats. Vomiting, they find, can be elicited when the lower brain stem is stimulated faradically. This effect is localized in the region of the solitary fasciculus and nucleus and in the dorsolateral border of the lateral reticular formation. Snider, McCulloch & Magoun (36) find that they can suppress reflexly and cortically induced movements by electrically stimulating the anterior cerebellar lobe and the

paramedian lobulus plus the nearby folia. Their evidence shows that the suppressor areas are practically coextensive with the facilitory and the tactile areas.

We may mention, finally, that work has been continued on the release of postural tremor (37). Monkeys with injury to the ventral tegmentum of the midbrain, dorsal to the central part of the substantia nigra, show postural tremor. If the lesion is unilateral, the tremor appears on the opposite side. Lesions involving the critical area but also extending beyond it did not produce the tremor.

An interesting paper by Peterson (38) suggests that the local application of acetylcholine to the cerebral cortex of the rat may influence the hand-preference in manual acts. Indeed, it may induce partial transfers in single-handed rats, an effect previously accomplished only by practice. The local application of acetylcholine may simulate the effects of forced practice through an excitatory effect upon the cortex. Peterson is rather cautious, however, in interpreting his results.

Studies on learning and the central nervous system.—Rosner & Neff (39) studied the role of the auditory cortex in the acquisition of a food-getting response in cats. In some animals, they did bilateral ablation before training; in others, a bilateral operation after training; and in still others, a two-stage operation with training between stages. All of the animals learned to respond correctly to auditory signals. Because Neff & Yela (40) earlier reported that such animals are unable to learn sound-localizations, they conclude that such a deficit is not simply a loss of ability to learn the significance of a sound stimulus.

The effects of hemidecortication have received some attention, and the results are rather interesting. Harlow (41), working with monkeys, has been studying their ability to form a learning set to solve oddity-principle problems. Extensive bilateral decortications, alone or in combination with lesions of the lateral surface of the frontal area, affect his animals more with respect to these solutions than do occipital lesions. This fact supports an earlier paper by Harlow & Settlage (42) showing that bilateral frontal lesion animals have as great a deficit as hemidecorticates.

Also involving hemidecortication are some experiments by Kellogg & Bashore (43, 44) with dogs. They studied its effects on conditioned flexion responses. With a technique of bilateral conditioning of the hind feet, they could compare affected and unaffected sides. Latencies of conditioned response, they find, are somewhat greater on the affected than on the unaffected side. Their results on the rate of conditioning indicate that animals which were allowed sufficient time to recover from the operations, showed much less deficit in rate of conditioning than those studied soon after operation. This fact calls for a reinterpretation of earlier work [see Bromiley (45)] in which hemidecortication seemed to retard the rate of conditioning. Apparently, earlier workers were studying the effects of temporary paralysis on conditioning more than that of hemidecortication per se. There seems to be an important point here—also one that is applicable to studies involving sen-

sory projection areas as well—that an operation may temporarily disrupt the sensory and motor capacities of an animal but that in time the animal may readjust to these deficits and be able to display relatively normal learning or conditioning.

This year has marked the publication of a study on "spinal conditioning" which most closely followed the original techniques of Shurrager & Culler (46, 47, 48). Pinto & Bromiley (49) studied 27 dogs, with spinal transections in the thoracic region, at postoperative times ranging from 3 hr. to 5 days. The CS were three shocks applied to the tail. The UCS was a shock to the left hindleg coincidental with the last CS shock. Responses were recorded directly from the semitendinosus muscle. Only one animal gave a response which could be called a conditioned response. The authors believe that even this was not a true conditioned response, but was due to a number of uncontrolled factors. These uncontrolled factors, which should be of considerable importance in future work on this problem, include: mechanical effects due to the movement of the foreparts of the dog, definite conditioning of the segment anterior to the transection in one preparation, change in the excitability of the isolated segment of the spinal cord, activation of the stretch receptors in the stimulated leg, etc. These conditions, which are largely secondary effects of the UCS, may explain many responses which occur to the CS or which occur spontaneously (to neither the CS or UCS). Deese & Kellogg (50) earlier showed that there is an unconditioned response of the semitendinosus muscle to shocks administered to the tail. Small variations in the intensity of the CS to the tail cause this unconditioned reflex to appear and disappear. The same effect presumably could be achieved by variations of skin resistance and sensitivity within the animal.

Among the various phenomena previously reported by the Shurragers (48) was one of converting a conditioned spinal reflex to a permanent reflex by cutting the contralateral spinal roots. We have long needed a control experiment to determine whether the "conversion" was one of a conditioned response or merely a change in reflexes produced directly by surgical manipulation. Pinto & Bromiley (49) have performed the control experiment and have given evidence for the latter alternative. After unilateral section of three lumbar and sacral roots, they observe a large drop in the threshold of the semitendinosus response to a tail shock—a procedure that follows that of the Shurragers but omits the so-called spinal conditioning.

The Shurragers, of course, had an opportunity to reply to the paper of Pinto & Bromiley, and did so (51). They construe the results of Pinto & Bromiley as supporting their original findings of "spinal conditioning," and question certain interpretations that Pinto & Bromiley made of the original studies of Shurrager & Culler. Certainly the issue is not closed, but to the reviewers, it seems that the evidence against spinal conditioning, at least the sort described by the Shurragers, is getting more and more convincing.

An interesting report from the Russian literature was available to the reviewers only through an abstract (52). It tells of a sharp increase in the difficulty of conditioning to light caused by extirpation of the cerebellum in fish.

Simultaneous removal of the forebrain and the cerebellum did nothing more than removal of the cerebellum alone. The authors conclude that in fish, the cerebellum is the dominant organ governing the functional capacity of the central nervous system. The obvious control of removing the forebrain alone is not reported in the abstract, so we cannot properly evaluate the conclusion.

Some theoretical considerations.—For some time, physiological theory has been inhibited by the recent emphasis upon molar behavior as the object of study. One of the most impressive events of the past year, however, was the publication of Hebb's book, *The Organization of Behavior* (53). In this, there is an interesting revival of theories relating learning to hypothesized conditions in the central nervous system.

This book is many things, but chief among them, as its subtitle implies, is a neuropsychological theory. Indeed, one of its most gratifying features is a valuable synthesis of stimulus-response and physiological theory with field and Gestalt theory. Very early in the book, Hebb rejects direct sensory control of behavior. He points out that such factors as set and attention imply an autonomous central process which biologically supports them. He points out that there is ample evidence to support such a notion [for instance, the electroencephalogram (EEG)].

On the problem of perception, Hebb departs from traditional Gestalt theory. His objective is "to show that 'simple perceptions' are in fact complex; that they are additive, that they depend partly on motor activity and that their apparent simplicity is only the end result of a long learning process" [(53) p. 17]. Thus he reverses the trend initiated by Lashley's work and emphasizes the dependence of perception upon the excitation of specific cells. In his argument, Hebb draws very heavily on the monograph by Senden (54) on the return of vision in the congenitally blind.

The crux of Hebb's argument comes in the "cell-assembly" theory, something which will undoubtedly be the subject of much future debate. "It is proposed first that a repeated stimulation of specific receptors will lead slowly to the formation of an 'assembly' of association-area cells which can act briefly as a closed system after stimulation has ceased" [(53) p. 60]. Hebb points out that reverberatory circuits could account only for very short and evanescent memories. He suggests that the primary function of such circuits is to maintain a "trace" until secondary growth changes can make it permanent.

Hebb develops this idea in some detail and then applies it to problems in the integration of perception, learning, memory, and attention. In the last half of the book, he enlarges his scope to treat motivation, emotion, and intelligence. Hebb's critics will find ample material on which to work. Despite the fact that much of his theorizing may seem to consist of pulling ideas out of the blue, the improvement over similar theorizing of 20 years ago appears to be very impressive. The book has in it a wealth of ideas, and it is certain to stimulate many more.

A much more restricted physiological theory of learning is found in a paper by Katz & Halstead (55). They advance a theory which makes protein

arrangement in nerve cells the substrate of the "memory trace." The authors discuss in some detail the structural features of proteins and their relations to nerve function. They admit and discuss the fact that their hypothesis may involve considerable revision of the current nerve impulse theory. The paper suggests several ways in which a protein lattice might participate in nerve conduction.

More important for psychologists is a detailed account of how a protein lattice could be important to the formation of traces immediately responsible for memory. A nerve, Katz & Halstead suggest, becomes a functional member of a network when a new, specifically oriented, protein molecule is formed. The molecule is a nucleo-protein and acts as a template for the synthesis of protein replicas (as in gene theory). Neurons of the cortex presumably have a large number of protein molecules in random configurations. The effect of the stimulus is to cause one of the randomly oriented molecules to assume a specific configuration. To test their notions, the authors suggest in a very brief outline some possible experiments.

The whole account is highly speculative, though there is nothing in it which is contrary to known biochemical properties of nerve tissue. The detailed reasoning and very specific statements make the theory almost unique among physiological theories of learning. Certainly the ideas deserve considerable attention and discussion. It is interesting to note that the theory of Katz & Halstead is not necessarily contradictory to Hebb's theory. As a matter of fact, the two could complement one another very well.

Studies on brain damage in humans.—There are two main sources of information about brain damage in humans. As one source, we have the widespread use of injury to the central nervous system for therapeutic purposes and the surgical removal of various brain neoplasms. As another source, we have a fairly large group of individuals who suffered brain injury as a result of the late war. Neither of these supplies us with very systematic information but they are now telling us more than has ever been available in the past.

Several recent papers have reported large scale testing of sensory capacities in brain damaged subjects. In two papers, Teuber & Bender (56, 57) report disturbances in vision following occipital damage. Deformation and disturbances in contour perception occur in such subjects, as well as changes in critical flicker frequency and disturbances of the perception of apparent and real motion. The results are not easy to interpret, particularly in view of the fact that knowledge of the extent and locus of the damage is somewhat hazy. Perhaps of greatest interest is the ability of the subjects to complete a figure if less than half of the target is projected on a blind area. In their interpretations, the authors emphasize "extinction" and an increased inhibition between hemispheres in brain damaged subjects.

Also, from brain-injured patients, Teuber, Bender & Battersby (58) report the effects of parietal lesions upon weight discrimination. Apparently damage in the parietal lobe does not impair the differential threshold for lifted weights. Such brain-damaged subjects do not differ from normal controls, nor do their affected sides differ from the unaffected sides unless they

have also some motor impairment. These results need not surprise us in view of the existing data on localization of kinesthetic discrimination in the monkey and chimpanzee, yet, because we lack exact knowledge about the lesions involved, we cannot be too confident about them. This the investigators know full well.

The most ambitious project in recent years on the effects of brain damage in human subjects is that recently reported by the Columbia-Greystone Associates (59). This large scale study grew out of the use of surgical techniques in the treatment of behavioral disorders. The approach was definitely experimental. Forty-eight subjects were intensively studied by a team consisting of every type of scientist who presumably could contribute something to the study. Twenty-four patients were controls, and 24 experimentals were subjected to a variety of bilateral partial frontal ablations called "topectomies." In the 24 operated subjects, the 11 Brodmann areas rostral to area 4 were systematically removed in various combinations so designed as to avoid artifacts and give information about each important area involved. The data are so summarized as to provide information concerning anatomy, physiology, psychology, and psychiatry. In addition, individual contributors report the results of particular tests or techniques, such as word-association tests, audiometric findings, visual findings, electroencephalography, learning and retention, intelligence, abstraction, Rorschach, affectivity, etc.

Of various physiological functions, none was affected by the different ablations with the possible exception of convulsions. One case, however, showed a clear-cut convulsive pattern; it had a lesion close to the motor region (middle frontal gyrus). In general, there seemed to be a decrease in activity (apathy) following operations, but this was only an impression. Also various examiners had the impression that the patients were more distractable following the operation. On the specific psychological tests there were very few detectable changes. A few temporary and isolated changes were suspected, though these did not reveal any consistent or meaningful pattern. "The amelioration from psychosis and social improvement which occurred in many of the operatees is easiest understood as an indirect effect of the operation" [(59), p. 494].

The findings in this study are almost uniformly negative. They have the function, however, of pointing out that there is no existing theory of the function of frontal areas in human beings which seems to flourish in the presence of a good deal of relevant data. The authors of this study were able to demolish a considerable number of notions about the functions of the frontal lobes, as well as to point in a positive way to fruitful avenues for future research.

In addition to the Greystone study, a number of individual clinical studies of patients (with frontal lesions) were reported during the past year. Their findings do not agree very well. Most reports say that patients are improved by various frontal operations (60, 61, 62). The primary mechanism for such changes, Meyer & McLardy (60) believe, is an alteration in the thalamo-prefrontal projection. This conclusion is based on a study of the

brains of 95 frontal leucotomy patients. Decreases in many intellectual factors were noticed by Petrie (62) in an examination of 20 patients following prefrontal leucotomy.

EEG and behavior.—The work on electroencephalography is bewilderingly large. To it an entire chapter (63) was assigned in the *Annual Review of Physiology* for 1949, a chapter which includes 266 references covering a two year period. Inaugurated in 1949 is a new journal, *Electroencephalography and Clinical Neurophysiology*, which is devoted largely to studies on the electrical activity of the brain. The various symposia in this journal and Walters' review paper indicate that research in this area is passing from the purely observational to the experimental and theoretical stage. The present review will cover only some of the papers on electrical activity of the brain which seem to be of interest to psychologists. These papers represent only a small fraction of the total number of foreign and domestic publications in the field.

Probably the item of most interest to psychologists is the newly discovered "kappa" wave, which seems to be related to thinking. The kappa wave is an intermittent spindle-shaped burst with an average frequency of from 8 to 12 cycles per sec. and average amplitude of 20 μv. The data on this wave have appeared only in brief papers and abstracts of papers read at meetings (64 to 67). The discoverers of this effect, Kennedy, Gottsdanker, Armington, and Gray, have performed a series of control experiments which rule out many of the possible artifacts that tentatively could be considered responsible for the effect (64). They have rejected the hypothesis that the kappa wave is a spread of alpha activity of the occipital region into the lower temporal region. Also they point out that kappa waves cannot be due to electrical activity of sense organs or muscles. Finally, they find that the most favorable position for recording kappa rhythms is on the temporal fossae.

The kappa rhythm is important because it seems to correlate with certain kinds of psychological activity. In one paper, Kennedy *et al.* find a relation between kappa rhythm and verbal report during problem solution (65). Bursts of kappa activity characteristically occur during the presolution period. In another study, the same authors report that the kappa rhythm is maximally shown during active recall of a partly learned task. During the period of initial learning of a task, however, there is little kappa activity, nor is there much during the recitation of very familiar material.

It is too early to judge the importance or generality of the kappa rhythm. It is unique, however, in that it is one of a few kinds of spontaneous electrical activity of the brain which is influenced in a positive way by mental activity. One may hope that the Tufts College group will give a detailed report of their researches at an early date.

The effects in human beings of rhythmic sensory stimulation (visual flicker) have been extensively described by Walter & Walter (68). In their studies, intermittent light gives a series of discrete elementary evoked responses. When these evoked responses fuse, they look like spontaneous activity. Evoked and spontaneous responses can summate. The evoked re-

sponses can be "driven" at the frequency of a visual stimulus. The investigators report finally that changes in the evoked responses correlate with various somatic, emotional, and mental changes.

Gibbs & Knott (69) present some comprehensive data on the growth of electrical activity with advancing ages in man. In general, there is an increase in the component voltages in successively faster frequencies from birth to 29 years. In the neonate, the primary components are from 1 to 3 cycles, but with increasing age, this band becomes less important. The band of from 4 to 6 cycles increases from birth to two years. Beyond this age, there is a steady increase in the 10 to 12 cycle band. A very high incidence of abnormal EEGs, Kennard (70) reports, appears in the neurotic or psychotic members of families with a relatively unstable psychological background.

There are many interesting papers dealing with the theory and origin of the rhythmic spontaneous activity of the brain, but we can review only a few of them. Kristiansen & Courtois (71) find that a spontaneous, Alpha-like activity can still be recorded following cortical isolation. This does not support the view that the spontaneous activity of the cortex necessarily depends upon a thalamocortical circuit—the view most prominently advanced by Darrow (72). There is much conflicting evidence on this point, however, hence the issue is not settled. Lindsley *et al.* (73) advance an hypothesis about wakefulness and the EEG desynchronization that results from afferent stimulation. Their hypothesis states that this effect may depend upon the activity of the lower brain rather than upon direct afferent influence arriving at the cortex over the classical sensory pathways. They studied cats with large lesions of the midbrain or diencephalon, and after destruction of the midbrain tegmentum or hypothalamus observed that the animals appeared to be asleep. At this time, their EEGs exhibited slow waves and spindles. Only strong stimulation evoked motor responses and desynchronized the EEG. On the other hand, after the medial and lateral lemnisci and spinothalamic tracts were interrupted in the midbrain, the animals were not abnormally sleepy and showed typical waking electroencephalograms. Thus, Lindsley and his colleagues have demonstrated the importance of the tegmentum and hypothalamus in maintaining wakefulness and in the arousal of the EEG by afferent stimulation.

A similar type of study upon cats and monkeys by Lindsley, Bowden & Magoun (74) also supports the notion that the hypothalamus and reticular formation play a part in the activation of cortical pattern. With transections at progressively higher levels, cortical activation decreased while synchronized activity of the EEGs increased. Thus it seems that when thalamocortical circuits are set free, they reverberate and develop periodic synchronization similar to the alpha pattern or to wave activities during sleep and barbiturate anesthesia.

Among the many symposia published in the first year of the journal, *Electroencephalography and Clinical Neurophysiology,* is one on the mechanism of spread of electrical activity and epileptic discharge (75, 76, 77). McCulloch (75) and Gerard (77) discuss ways in which the EEG is propa-

gated as well as the function of the synchronous activity of the nervous system and the meaning of desynchronization.

There were many papers during the past year describing improvements in apparatus and procedure in recording EEGs. Of particular interest to psychologists is a paper (78) which describes the radio transmission of the human EEG as well as other electrophysiological data. The technique is not reported in detail, but it is apparent that this procedure greatly increases the range of the subject's activity during which recordings may be made.

Convulsive Behavior

Psychologists have a great interest in experimentally produced, convulsive behavior. In the recent literature, production of convulsions by electroshock has supplanted to a very considerable extent the earlier work on audiogenic seizures. This represents both a change in theoretical interest and a change brought about by widespread clinical use of electroshock therapy. Indeed, in the forefront now are the problems of the lasting effects of electroshock convulsions on learning and on emotional and instinctive behavior.

Convulsions and learning.—There is a sizable literature on the effects of electroconvulsive shock on various aspects of learning. This material is almost entirely of an empirical nature, so it lends itself to a simple, if not very coherent summary.

Studies from the Pittsburgh group show that electroshock convulsions make it more difficult for rats to learn a Lashley-type water maze both 24 hr. and 30 days after electroshock treatment (79). There is likewise a more or less permanent impairment in retention of the Lashley-type water maze (80). The magnitude of the decrement in retention does not appear to be related to the intensity of the electroshock used to induce convulsions (81). The decremental effect of electroshock convulsion does seem to be related, however, to the difficulty of the maze used (82).

Among the studies from other laboratories, one by Muhlhan & Stone (83) shows that the preference for the more economical routes in a Dashiell-type water maze is disturbed by electroshock convulsions. Siegel & Siegel (84) have evidence which shows that the anticipatory gradient (measured in running time) in a runway appears to be abolished by electroconvulsive shock. Rats subjected to electroconvulsive shocks show a decrease in running time in a runway, but nembutal, which prevents the convulsive reaction following shock, also prevents the appearance of this decrease in running time (85).

Worchel & Narciso (86) present additional data on the effects of electroconvulsive shocks on human learning. Data on nonsense syllable learning in two patients support the Mayer-Gross (87) conclusions that a single electroconvulsive shock does not obliterate memory of material learned immediately before shock. After five shocks given over a period of eight days, however, the subjects were unable to learn a series of 10 nonsense syllables. There was no saving on a relearning test. The authors conclude that the effects of a series of electroconvulsive shocks are cumulative, but gradually disappear after the shock treatments end.

Convulsions and sexual and maternal behavior.—Maternal behavior is abolished by electroconvulsive shocks given during the last 11 or 12 days of pregnancy and one day after the litter is born (88). Shocks during the first 15 days of lactation seriously disturb, but do not eliminate all maternal behavior (88). If shocks start after nestbuilding and lactation have been well established (7th day postpartum), however, there is little disruption. Comparisons with other studies lead Rosvold to believe that the effects of shock on pregnancy and maternal behavior are greater than can be accounted for by known or hypothetical damage to the cerebral cortex (89).

Ether anesthesia, which prevents convulsions, protects animals against the damaging effects of electroshock on maternal behavior (90). This finding is in accord with similar protective results of anesthesia on the effect of electroshock on learning behavior (91, 85).

Jensen & Stainbrook relate electroshock and pseudopregnancy. They show that the electroconvulsive shocks produce pseudopregnancy (92), as also will electrical stimulation of the cervix (93). They recognize, however, that it is not clear whether or not the effects of an electroshock convulsion are produced entirely by central mechanisms. Hyperphagia also results from continued electroshock convulsions (92).

Audiogenic seizures.—Contrary to an earlier report by Patton (94), Kenshalo & Kryter (95) find no difference in incidence of audiogenic seizures in rats without middle ear infection, rats with one ear infected, and rats with both ears infected. Rats with both ears infected, however, give a greater frequency of seizures than do the other rats. Pilgrim & Patton (96) have shown a high incidence of audiogenic seizures to be associated with pyridoxin deficiency when the deficiency is induced in suckling rats by feeding a deficient diet to the mother and continued by giving the same diet to the young after weaning. The resulting susceptibility to seizures can be reversed or prevented by administration of pyridoxin.

Griffiths (97) studied the effects of adrenalectomy on the incidence of audiogenic seizures among domestic and wild rats. He was particularly interested in the hypothesis that lethargy and nonemotionality are factors which increase susceptibility to sound-induced seizures. Activity levels, indices of emotionality, and susceptibility to seizures were measured before and after adrenalectomy. The results throw doubt on the hypothesis relating lethargy to susceptibility. The wild rats showed a drop in activity and emotionality scores following adrenalectomy although they maintained their original resistance to seizures. Essentially the same results were obtained on the seizure-resistant albinos. Griffiths & Cohen (98) find that neither morphine sulphate nor diaminon hydrochloride significantly affect the incidence of audiogenic seizures in albino rats.

Maier (99) published a book which summarizes his many experiments on frustration and behavior. He stoutly maintains the position, earlier demonstrated in an experiment by Maier & Longhurst (100), that audiogenic seizures are produced by, or at least facilitated by, a conflict situation. Despite the fact that this point does not seem to be crucial to the main arguments of

Maier's book, the question deserves further experimental work. It is clear that the primary cause of such seizures is the auditory stimulus (101). Maier's data (99) seem to indicate that such sound-produced seizures may be modified by "psychological" variables, but the question as to whether this is actually the case has now been argued for a decade without final settlement.

Patton, Russell & Pierce (102) observed a decrement in electroconvulsive thresholds when auditory stimulation preceded the administration of shock, even though the auditory stimulation in itself did not induce seizures.

Convulsive behavior and physiological conditions.—Why convulsions, particularly those induced by electroshock, affect behavior is still very much a mystery. Electroconvulsive shock apparently disturbs behavior more than one would expect from the observable damage done to the central nervous system (89, 103). Furthermore it seems very clear that the electroshock must cause a convulsion in order to produce a behavior deficit (85, 90, 91). This fact may make us suspect that convulsions and the numerous behavioral deficits produced by them are related by way of a common cause. We already know something about the neural and chemical basis of convulsions [see Morgan & Stellar (104)], but it is probably worth while to review some of the new evidence on the causes of the seizure state.

Sodium seems to be an important factor. Davenport (105) and Davenport & Woodbury (106) show that the electroshock threshold itself probably depends upon the level of plasma sodium. If the level of plasma sodium is high, the electroshock threshold is high; if the plasma sodium drops below a certain concentration, there is a drop in the electroshock threshold—as a result, presumably, of increased excitability. Despite the changes in plasma sodium, the brains of adrenalectomized rats and rats injected with various salts and desoxycorticosterone preserve the normal content of sodium and potassium.

Acetylcholine is also a factor to consider. In an experiment by Hyde, Beckett & Gellhorn (107), for example, eserine administered intravenously and applied directly to the cortex facilitated the convulsive response to eight different, unrelated chemicals. Further observations show that convulsive activity is diminished by local application of cholinesterase. These findings indirectly indicate that acetylcholine plays an important facilitative part in the convulsive activity.

Other studies of the mechanisms and results of seizure states show that (*a*) convulsions result from reduced lipin and lipoid content of the brain, a fact which falls in line with the hypothesis that convulsions are normally related to a deficiency of brain cholesterols (108), (*b*) chemically induced seizures cause a smaller increase in lactic acid and a higher phosphocreatine level in the cerebellum than in the cortex, even when seizure-activities are about the same (109), and (*c*) there probably is a genetic factor in the mortality of mice from electroconvulsive shocks (110).

These studies point toward the eventual solution of the causes and contributing factors of convulsions, but do not tell why experimentally produced

convulsions are frequently associated with certain kinds of behavioral deficits. They make it clear, however, that seizures come about through profound disturbances of the chemistry of the central nervous system. It is possible that the behavioral deficits, in part, are produced by these same disturbances. Some of the behavioral deficits may be produced by "psychological" conditions of fear, etc., but it seems overwhelmingly evident that the major causes are "physiological" in nature.

Chemicals and Behavior

Motivation and behavior.—Perhaps the states of hunger and thirst have been more thoroughly explored than any other motive condition. Nevertheless, thirst and hunger remain among the most obscure of the biological motives. Hunger is especially complicated because there is not only the problem of the instigation of eating behavior, but also that of the selection of specific substances. Indeed, it is evident that thirst is only a special case of such selection. We are still trying to divine the mechanisms regulating water and food intake in the normal animal. Each of the studies to be reviewed here represents a small contribution to this end. This is worth keeping in mind since we can point out only briefly the connection of each study reviewed.

Do factors that affect hunger also alter taste and smell? Contrary to an earlier report by Goetzl & Stone (111), Janowitz & Grossman (112) were unable to relate a change in taste or olfactory acuity with changes in appetite in humans. As previously reported (113), a dose of D-amphetamine abolishes ability to eat, but it has no consistent effects upon olfactory acuity. The experiments by Janowitz & Grossman seem to represent the use of better experimental controls than those found in the earlier experiment.

What does food in the stomach have to do with hunger? In the intact dog, the ingestion of part of a meal 20 min. before feeding decreases intake by an equivalent amount (114). In gastrostomized dogs the introduction of food into the stomach has the same effect. Moreover, inert material is just as effective as food, which indicates that the effect is a mechanical one. If the intragastric feeding is given several hours before oral feeding, there is no decrease in oral intake. In esophagostomized dogs, sham feeding greatly exceeds the food deficit; yet if food is placed in the stomach some time before, it does not inhibit sham feeding. Sham feeding is inhibited to some extent, however, by gastric distension during eating. From these factors, Janowitz & Grossman conclude that the termination of feeding is brought about by a co-operation of oral and gastric factors. Simple distension of the stomach can cut down feeding behavior long before feeding behavior would have stopped with an empty stomach.

How are specific hungers regulated? Pfaffmann (115) demonstrated that salt preference in rats is modified by the level of thirst present. He suggests that the strong thirst drive may reduce the preferential discriminations of taste stimuli. Bare (116) studied the specific hunger for salt in normal and adrenalectomized white rats. He was able to verify Richter's observations

(117) that rats show a decrease in salt preference threshold following adrenalectomy, though he does not agree with Richter that this is a result of change in receptor threshold. Bare discusses two alternative theories, a reinforcement theory and a nonlearning theory to account for the specific hunger for salt shown by both his normal and adrenalectomized rats.

Before presenting theoretical notions about hunger and thirst, we shall mention briefly some other recent papers. Several investigators have studied the influence of hunger and thirst on activity. Seigal & Steinberg (118) find that activity is a negatively accelerated increasing function of hours of food deprivation. Finger (119) agrees with this in finding that food deprivation leads to increased activity, but when he restored animals to ad lib. feeding schedules, activity was depressed markedly below normal. Seigel & Seigel (120) find that the mean water intake of emotional animals is significantly greater than that of controls. Heron & Peake (121) were able to demonstrate that rats could learn a black-white discrimination in a Lashley jumping box when motivated by a protein deficiency.

Now we come to some theories about hungers. Young (122) has published a theoretical paper dealing with his and other research on selective hungers. To him, much of the dietary choice behavior is a matter of learning. The preferential food selections of rats are regulated by the intensity of effective arousals of food contacts and the number and temporal distribution of runs to food.

Also relevant to the physiological theory of hunger is the interesting "genetotrophic" theory of the etiology of alcoholism (123). The authors of this theory do not deny the importance of psychological and sociological factors in the production of alcoholism. They suggest, however, that an important predisposing factor in alcoholism is a genetically determined specific hunger. They speak of "genetotrophic disease" and define it as an inheritable characteristic of diminution of the ability to carry out some specific enzymatic transformation. The authors cite evidence to the effect that the metabolism of alcoholics is linked with the utilization of vitamins. Specific hungers for these vitamins based upon a partial genetic block in metabolism may induce a craving for alcohol. The theory is quite acceptable from the standpoint of genetics and biochemistry. Not much direct evidence is available to support it, however. The authors cite the fact that rats and mice on a stock diet exhibit individual preferences for alcohol. This preference can be altered by nutritional factors and seems to be associated with certain gross strain differences. There is also an interaction between diet and strain differences in producing perferences for alcohol. Furthermore, the ability to control appetite for alcohol by administration of 10 B-vitamins and the antipernicious anemia vitamin in the two strains is different.

The concept of genetotrophic disease has been extended by Williams *et al.* (124) to many different disorders. In general, the theory may be applied wherever it is suspected that there is a nutrient lack which has a genetic background—to cancer, diabetes mellitus, rheumatoid arthritis, disseminated sclerosis, and mental disease. Certainly the theory deserves wide atten-

tion in the behavioral sciences because it suggests a clear-cut path from the gene to behavior. The relationships between specific genes and body chemistry are being worked out in considerable detail. Also, the relationship between body chemistry and appetite has been the subject of much fruitful investigation. We know that appetite can be reflected in specific preferences by animals and humans. Thus, we are presented with the possibility of an unusually clear story from the gene to behavior. Certainly the genetotrophic theory will lend itself to some very interesting experiments.

Chemicals and learning.—We know, of course, that many substances are important in nerve metabolism. Furthermore, the supply of these substances depends upon the more general nutritional state of the organism. It has, therefore, been natural for workers to look for behavioral effects of supranormal or subnormal amounts of various nutritive substances known to bear directly on the activity of nerve cells. It might be supposed that by altering intake of these substances, changes in the level of nervous activity would result. These changes in activity could be reflected in a number of behavioral changes. The most significant change, of course, would be a change in learning and intelligence. Therefore perhaps optimistically, many workers have looked for change in learning ability in animals following certain types of dietary manipulations. There have been several studies following this line during the year.

O'Neill (125) studied the effects of graded amounts of vitamin B_1 in the diet of young rats upon subsequent maze learning ability. A previous unpublished study (126) from the same laboratory showed that older rats subjected to thiamine deficiency were not different from controls. In O'Neill's experiment, the controlled diet was begun six days after birth and continued until the rats were eight weeks old, at which time it was discontinued. Graded amounts of thiamine were fed eight different groups, and three different mazes were used to study learning ability. As a result, perhaps, of the large number of measures taken, the data are not clear-cut. However, they do seem to support O'Neill's conclusion that a severe thiamine deficiency significantly weakens maze learning ability but that a great excess of vitamin raises it well above normal.

Readers will probably remember the observation of Zimmerman & Ross (127) that glutamic acid has beneficial effects on maze learning ability in rats. Subsequent studies have failed to support them, and now we have two more negative studies to add to the list. Marx (128) presents a test of the hypothesis that supranormal quantities of glutamic acid may facilitate maze performance. In one experiment, he looks into the possibility that the diet which was used in studies obtaining positive results might have been low in glutamic acid and that the improvement noted may have been due to the addition of adequate amounts. This hypothesis is not confirmed by Marx's experiment. In a second experiment, he used very large doses of glutamic acid but still noticed no effect on maze performance. Porter & Griffin (129) have similarly been unable to show any effect either of a diet deficient in glutamic acid or of one high in glutamic acid. They also obtained no effect of glutamic acid upon the effect of electroshock convulsions.

The general agreement among many different experiments negating a relation between glutamic acid and maze learning makes it now seem likely that the original results of Zimmerman & Ross were due to an experimental artifact or to some such factor as a defective group of animals which would require excessive quantities of glutamic acid.

Endocrine functions and behavior.—Beach has added several papers to his series on the role of hormones and of the nervous system in the mating behavior of mammals. In one, Beach & Holz-Tucker (130) report the concentration of androgen which, when administered to castrated male rats, will keep sexual behavior at preoperative levels. This critical dosage varied somewhat with the behavioral measure used, but a dosage of from 50 to 75 mg. per day seemed to be effective. The authors point out that this conclusion is limited by a number of factors: the sample of rats used, the method of administration, and the particular form of the hormone used (testosterone propionate administered subcutaneously in sesame oil). The effect of administration of androgen (reduced latencies, etc.) agrees well with the hypothesis that one important function of the hormone is to lower the threshold in the nervous mechanism responsible for mating behavior.

In a second paper, Beach & Pauker (131) report the effect of castration and androgen administration on the male hamster. There are certain interspecies differences between the rat and the hamster. Copulatory behavior in the hamster survives castration much better than in the rat, even though the capacity for intromission is lost at about the same time. The rat maintains the ability to achieve intromission under hormonal concentrations too low to maintain ejaculation (130). In the rat, however, it was difficult to maintain the incomplete copulatory response without intromission; in the hamster, a low hormonal concentration will maintain the incomplete copulation response without intromission.

It has been observed that both natural and artificial estrogens inhibit growth in rats and mice. This fact is usually assumed to be due to one or several of some possible hormonal interactions. Meites (132) reports an experiment, the purpose of which was to determine how much of this growth-inhibition may be due to a reduction in food intake. He reached the conclusion that artificial estrogen (diethystilbestrol) can curtail growth by inhibiting appetite, while natural estrogens can inhibit growth without a corresponding decrease in appetite. The action of the artificial estrogens, he suggests, may be due to a vitamin deficiency (perhaps vitamin B_{12}).

Because of its practical importance, the relation between human behavior and hormonal activity has received much attention, particularly in the area of abnormal behavior. The study of this problem ranges from casual observation of clinical cases to full-fledged experiments. Again, because many of the better studies are parts of large, continued programs, we cannot hope to give an integrated picture of the total activity in this field.

Brody (133) has advanced an interesting hypothesis about the physiological basis of stress and tension. He notes a small difference in the concentration of precipitable serum iodine between patients who show "tensions" (inability to relax, profound reactions to stress, etc.) and those who do not.

Therefore, he suggests that repeated stressful situations may increase thyroid activity and that the higher thyroid hormone output in turn maintains increased neural sensitivity and thus also maintains higher tension levels.

Some of the most interesting and significant data on the relation of hormonal activity to personality types and behavioral disorders comes from the work of Pincus, Hoagland *et al.* (134, 135). These workers have clearly established a difference in reaction to adrenocortical stress between normal and schizophrenic men (134). Blood and urine constituents were determined before and after four tests designed to produce adrenocortical stress. They were a pursuitmeter test, a target-ball frustration test, a glucose tolerance test, and the administration of adrenocorticotrophic hormones. The responsivity of the patients was low except in the glucose administration, where a lymphocyte response appeared. The fact that the schizophrenic patient fails to react to both the psychological and the physiological stress seems to indicate that the lack of adrenocortical response is not due to the characteristic apathy of the schizophrenic. A subsequent paper (135) shows that nutritional factors cannot account for the lack of responsivity to the adrenocorticotrophic hormones.

Hoskins & Pincus (136) have compared hormonal relations in normal men and women with those in schizophrenic men by taking the ratios of the urinary output of 17-ketosteroids, androgens, and estrogens. The women secreted more estrogen, less androgen, and less of the 17-ketosteroids than did the normal men. In estrogen output, the schizophrenic men fell between the women and the normal men, so they seem to approach the feminine patterns in their hormonal relations. In the case histories of these patients, however, there were no signs that their hormonal make-up caused any trend toward homosexuality.

This type of research may prove to be of fundamental importance in the understanding of the psychoses as well as of normal personality characteristics. They do not necessarily imply, of course, that psychoses are secondary reactions to hormonal-nervous conditions, but they do imply that there is a necessary physiological substrate upon which the behavioral causes of various psychotic states must work. It is very possible, furthermore, that there is an important interaction of physiological conditions with behavioral conditions. A good example of this point is Brody's hypothesis (133) concerning the higher thyroid activity in people who are very reactive to stress. He implies that this higher activity may be maintained in these people, at least partly, by frequent and violent reactions to stressful situations.

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ABNORMALITIES OF BEHAVIOR

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Psychiatric literature for the preceding year has been reviewed in *The Yearbook of Neurology, Psychiatry and Neurosurgery* (1), *Progress in Neurology and Psychiatry* (2), and the 1949 "Review of Psychiatric Progress" by Hoch & Lewis (3). These summaries, which provide a selected bibliography, overlap to some extent but vary in emphasis. The 1950 *Yearbook of Psychoanalysis* (4) is a collection of representative, significant papers which were published separately in 1948. Other recent books of broad scope include Wolff's (5) comprehensive survey of psychopathology employing dynamic concepts. New editions of Allen's (6) *Modern Discoveries in Medical Psychology* and Glover's (7) psychoanalytic handbook were issued. White (8) and O'Kelly (9) present introductory textbooks on abnormal psychology. O'Kelly's book suffers from oversimplification and confusion in basic concepts. A new textbook of psychiatry is edited by Rees (10). The various chapters, contributed by authorities in England and America, cover a wide range of topics.

Psychodynamics

The general subject of psychodynamics is discussed in a recent monograph by Weiss (11). Much of the current literature continues to emphasize the dynamic interaction between the individual and his environment in determining disordered behavior, as well as satisfactory adjustments. With such an orientation as a starting point, Marmor & Pumpian-Mindlen (12) attempt a comprehensive formulation about mental disorders. They conclude that everyone is susceptible to a mental illness and can suffer from a different type of disorder at different times. Various forms of disorder merge into one another by a series of gradations. This often made assertion implies that one disorder on the continuum is quantitatively different from the next. If by quantitative difference is meant difference in severity of the disorder or quantitative differences in the mental mechanisms, the concept is at variance with clinical experience. In another broad approach to psychopathology, Cameron (13) suggests using the concept of social roles. A further expression of the emotional aspects of interpersonal relations is presented by Thompson (14). He states that in most mental disturbances there is a fundamental disturbance in the capacity for relationships. This disturbance is erected as an alternative to intolerable anxiety engendered in early affective troubles.

Explicitly or implicitly, discussions of disordered personal relationships often refer to some aspect of difficulties in early family life. For example, English (15) discusses how the family affects the individual. In a study of

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college girls, Ellis (16) shows that their subsequent love experiences are related to the quality of affection experienced in family life. Brower (17) reports that the families of rejected children are characterized by the failure of the adults to give these children their basic rights to love, security, and a sense of belonging. These children are not given the feeling that they are accepted for their own sakes. Kanner (18) similarly notes the detrimental effect of lack of parental emotional warmth. His study of 55 autistic children revealed that most were exposed to a cold, obsessive, mechanical kind of parental care centered on physical needs only.

In contrast to the previous year, there have been few publications on stress as a pathogenic factor. However, Gramlich (19) reports on a psychological study of stress in military service, and Honigmann (20) discusses the relation between culture patterns and human stress. The postwar problems of displaced persons have presented a setting for investigations of social and cultural stresses on large groups. Friedman (21) reports his clinical observations of displaced persons in Cyprus. It is his impression that in the desperate struggle for survival these people mobilize all the forces of the libido around the need for self-preservation. Wolman (22) analyzes the acculturation difficulties of the Oriental Jew in Palestine, where the kind of social pressure is new to him.

Considerable attention has been focused on the factor of anxiety in psychodynamics. A most comprehensive treatment of anxiety is presented in May's (23) scholarly book. The first part of the book is general in scope, covering the various approaches to the problem. The second part presents a clinical analysis based on case studies. From his multiple approach, May concludes that "neurotic anxiety results from a cleavage or contradiction between expectation and reality, a contradiction which occurred originally in the person's relations with and attitudes toward his parents." This cleavage, it is pointed out, has a healthy as well as a neurotic form and is one of the conditions for creative activity. Anxiety, then, if handled in neurotic ways leads to distortions of reality and to symptoms, whereas normally it can lead to healthy, creative activity.

Fischer (24) differentiates insecurity and anxiety, which are traceable to early childhood experiences. Insecurity arises from an unsatisfied craving for protection and becomes a continuous state penetrating every aspect of life. Anxiety, on the other hand, comes from an experience of complete helplessness in the face of real or imagined danger, appearing only in certain situations. The adult neurotic anxiety and insecurity appear with a mixture of distressing feelings.

Many of the other articles in the area of psychopathology make some reference to anxiety. The general impression is that it is an affective experience, conscious or unrecognized, which arises from some conflicting mental activities. Some authors then speak of anxiety as a symptom in itself, while others indicate that what is manifested clinically is the defense against the anxiety. There is need for further clarification of the widely used concept of anxiety.

Dynamically related to anxiety are frustration, aggression, hostility, and guilt. These subjects continue to receive some attention in the literature. Pastore (25) re-evaluates the frustration-aggression hypothesis of the Yale group. They had shown that frustration initiates a hierarchy of response tendencies, one of which is aggression. Pastore agrees that frustration may lead to aggression, but he maintains that there is an intermediate factor. Aggression is the reaction to the unreasonableness of the situation rather than to frustration. In support of his contention, he reports his experiments in which subjects went without sleep, following which promises which had been made to the subjects were not kept. Pastore assumes that the subsequent aggressiveness was a result of the broken promises which seemed unjustified to the subjects. It is, however, questionable whether the sleeplessness in this experiment was frustrating in itself or whether the subjects were frustrated only by failure to realize promises held out to them.

Freud (26) reviews two chief analytic theories of aggression: as arising from the frustration of the ego instinct, and as a distinct instinct of equal status with sex. Neither sex nor aggression can be observed in pure form as they combine with or oppose each other. The fusion of sex with aggression makes it possible for the child to assert rights to possession, compete with rivals, and satisfy curiosities. Carrying out the sexual act in adult sex life presupposes sufficient aggression to obtain mastery over the sexual partner. In abnormal cases, where the admixture of aggression and sex is deficient, sexuality becomes ineffective and impotent. On the other hand, when not fused with sex, aggressive urges result in purely destructive tendencies. Aggressiveness as abnormal behavior is discussed by Owen & Stemmermann (27). It is noted that aggressiveness may take the form of active combativeness or be manifested by passive resistance. Because aggressive behavior in the mentally ill is likely to generate further feelings of guilt, the authors believe aggressive expression should be circumvented. Apparently they believe that further repression or suppression is more therapeutic than expression of aggression in a controlled situation where it may be accepted and not engender more guilt.

Most reports on hostility are in reference to children. According to Cushing & Cushing (28), the genesis of hostility may be during prenatal life, if conducive circumstances exist. After birth, the reaction of hostility is simply a continuance of the earlier pattern. Even to those who would take this formulation seriously, it is subject to considerable question. Korner (29) reports her study of hostility in young children. Her book gives an account of observations made upon children, but offers no conclusions.

Except in connection with other problems, there have been few articles about guilt. However, on the basis of analyses of case histories and dreams, Choisy (30) shows that feelings of insecurity and guilt are not related to sin in the theological sense. Here and there efforts have been made to clarify the difference between the psychological meaning of irrational guilt feelings and the ethical meaning of guilt associated with outward wrongdoing. This distinction is touched upon in a provocative paper by Hsu (31) on suppres-

sion and repression in relation to the culture. For example, he distinguishes between "guilt" and "shame" cultures, cultures in which inner conformity and repression predominate, and cultures more characterized by outer conformity and suppressions.

Repression as an active mental process is reviewed by Zeller (32) from the point of view of experimental psychology. He finds no experiment in the literature crucially fulfilling requirements to demonstrate repression. Perhaps his experimental criteria specifically exclude clinical data, including hypnosis, wherein repression is regularly demonstrated. Another mental mechanism given some attention is sublimation. Lee (33) discusses Freud's concept of sublimation. Lictenberg (34) also reviews Freudian and other theories of sublimation. Disagreeing with these formulations, he offers his own concept, that "sublimation differs from neurosis in the amount of repression involved and in the placement of repression."

A number of other articles report on various concepts of dynamic processes. Among these is a paper on ego functions and dreams by Mittelmann (35). Gross (36) discusses two ways in which the sense of time is manifested in dreams. Hart (37) discusses various symbolic meanings of the eye and symptoms based on those symbols. A paper on oral mechanisms is presented by Schaffer & Keiser (38). Rashkis (39) discusses factors in disordered thinking. Other papers on psychodynamics are noted in connection with specific psychopathology.

Psychosomatics

As a continuation of the recent trend, there is a strikingly increased interest in psychosomatic problems in the literature for the past year. The area of psychosomatic disorders has been approached from two principal directions. One, largely represented by psychoanalytic studies, has been the dynamic investigation of the personality of patients. Most of the literature is in this category. The other approach, principally experimental and physiological, attempts to demonstrate and correlate visceral or somatic changes induced by specific emotional states.

Among the general works published is a second edition of the popular Weiss & English (40) textbook on psychosomatic medicine. This book, useful as a reference, presents a comprehensive view of the field. Seguin (41) presents an excellent elementary general introduction to the field.

A number of general, orienting articles on psychosomatics appear in the literature. Among these is Kauders' (42) discussion of psychosomatic interrelationships. Menninger's (43) paper on emotional factors in illness is addressed primarily to physicians. He points out that emotions may be causative factors, resultant symptoms, or complications of organic disease. Jason (44) also discusses this aspect, emphasizing that many physiologic functions are easily influenced by emotion. These emotional states which disturb bodily functions are more specifically described by English (45). He lists anxiety, need of love, hostility, inferiority feelings, guilt, ambivalence, ambition, envy, and their related feelings. Another formulation of this aspect

is provided by Stalker (46), who points out that the emotional level may be conscious and unconscious and that the response can be voluntary or involuntary. He suggests that aggressive trends are more dominant and more frequent in psychosomatic states than in other psychiatric conditions. Although most interest has centered about unpleasant states, Stevenson (47) notes that bodily symptoms may arise during highly pleasurable emotional states. He recognizes, however, that the conscious experience of joy and elation immediately preceding physical symptoms may be associated with unconscious emotional disturbances.

The correlation of symptoms with personality type is briefly reviewed by Lhamon & Saul (48). They emphasize that it is not the type or the personality as a homogenous unit which is related to somatic disorder. Rather, the correlation is with the typical emotional force or typical conflict situation involving partial regressive mechanisms. Thus, so long as these mechanisms may operate in very different personalities, psychosomatic symptoms occur in all sorts of personalities. Gildea (49) expresses a similar point of view and presents the special patterns of personality malfunction which are common to certain psychosomatic disorders. His paper integrates many of the findings reported in the literature. Another illuminating aspect of this article is the consensus that many patients are resistive to psychotherapy. Bettelheim & Sylvester (50), in a study of physical symptoms in emotionally disturbed children, show that physical symptoms come and go as steps in the integrative process of emotional readjustment in these children. Somatic symptoms were found to be unrelated to specific conflicts or personality patterns, but related to the child's maturity, to integrative tasks of the moment, and to the attitude of the parent figure to the somatic symptom. Further efforts to delineate characteristic personality factors and conflict problems were made by Modell & Potter (51), using human figure drawing as a projective technique. A Jungian emphasis in psychosomatic studies is offered by Kirsch (52), who urges investigation of instincts and the relationship of consciousness to the collective unconscious.

Many somatic syndromes have been investigated from the psychological standpoint. There is even a paper by Davidson (53) discussing emotional precipitants of death operating through cardiovascular tensions. Whereas most of the earlier studies on circulatory disturbances were focused on arterial hypertension, heart diseases are now receiving attention. Storment (54) discusses personality and heart disease. Williams (55) relates the conflict between strivings to dominate and the need to be loved to coronary occlusion in a patient. In studies of patients with cardiac arrhythmias, Stevenson *et al.* (56) and Duncan *et al.* (57) found that they showed certain personality features in common. These characteristics rendered the patients unusually vulnerable to stressful life situations, to which they were likely to respond by developing sustained anxiety, resentment, conflict, and depression. The life situations were thus found to be relevant to the occurrence of cardiac arrhythmias. Josselyn (58) found that children recovering from rheumatic heart disease tend to develop anxieties. These anxieties, in turn, may exert

a damaging effect upon the heart. In another paper on neuro-circulatory phenomena, Evans (59) notes that the carotid sinus syndrome may be related to nervous strain.

Of gastrointestinal disorders, perhaps the most extensively studied is peptic ulcer. Sullivan & McKell (60) present a book on personality factors in peptic ulcer patients. Personality factors as indicated by interpretations of the Rorschach test are reported by Brown *et al.* (61). Their findings suggest that ulcer patients as a group tend to deal with their environment at an impulsive, immature level. In a reply to an earlier article by Mahl (62), Szasz (63) critically refutes the assertion that the most important factor in the pathogenesis of peptic ulcer is chronic anxiety or fear. Szasz (64) studied a number of patients who had been treated by vagotomy. It was found that these patients generally developed other signs of "exacerbation of symptoms (defenses) which had been employed by the patient some time in his past." The more important psychologically and the greater the emotional investment in the ulcer symptoms, the more likely were the results of vagotomy to be unfavorable.

Other studies on gastrointestinal disorders include a report by Halsted *et al.* (65) on emotional aspects of cardiospasm. Spastic colitis, which has been studied intensively in the past, is discussed further by Fest & Seward (66). Sterba (67) presents a case study of a child with constipation which was a temporary reaction to severe toilet training.

Lehman (68) reviews the literature and discusses psychogenic feeding problems. The psychological factors in overeating are summarized by Rynearson & Gastineau (69) in a book on obesity. It is noted that the social consequences of obesity tend to perpetuate, accentuate, and add to the original emotional determinants of overeating.

Psychosomatic problems in nose and throat disorders are discussed by Coleman (70) and Wolf *et al.* (71). In an analysis of neurogenic factors in nasal congestion, Fowler (72) suggests that many colds are induced by psychic stimuli and that all sorts of nasal catarrh can have psychosomatic factors in the background. Metzger (73) has observed in the records of allergic patients that at least half of asthma and hay fever patients have psychoneurotic components in their complaints. Complicating emotional factors are considered. By an intensive study of a patient with chronic bronchitis and bronchiectasis, Stevenson & Wolff (74) have demonstrated that during stressful life situations bronchial mucus increased markedly. When the stress was over, the secretions again diminished.

Among the several articles on skin conditions are those by Klein (75), Gottesman & Menninger (76), and Dunkel (77), who point out some of the characteristics of patients with neurodermatitis. Dunbar *et al.* (78) present in the form of a conference report a study of a man with chronic atopic eczema and asthma. The discussion points out how this patient has utilized his dermatitis and scratching to discharge tension and as an expression of certain repressed trends in his personality. Sperling's (79) analysis of a child

reveals how unresolved and unexpressed hostility in this child resulted in recurrent ulcer of the leg with other somatic symptoms.

The dynamics of migraine headache attacks is formulated by Marcussen & Wolff (80). They conclude that the migraine represents a way of dealing with life situations which are too stressful to the individual. Thus, when the individual has reached the limit of his capacity to tolerate tension, hostility, frustration, and fatigue, one of the protective reactions is the headache which forces him to halt.

Psychiatric aspects of low back pain are discussed by Bendheim (81), Solomon (82), and Paul (83), who review the literature on the subject. Paul summarizes the various psychodynamic mechanisms, such as conversion phenomena or muscular expression of anxiety-tension states.

Metabolic disturbances are also being investigated. Following a preliminary survey of emotional factors in the etiology of hyperthyroidism, Lidz & Whitehorn (84) describe efforts to apply these concepts in treating thyroid patients. Among the studies of diabetics are those of Bruch (85), Hinkle & Wolf (86), and Rosen & Lidz (87). Different investigators agree that diabetics apparently do not have distinct personality features in common. However, emotional upsets are likely to throw the diabetes out of control and to precipitate acidosis.

A number of miscellaneous studies of psychosomatic relationships have appeared. For example, Sicher (88) discusses the climacterium, emphasizing social and cultural attitudes toward the involutional period. Articles by Vics (89) and Wong (90) are on psychiatric aspects of ocular complaints. Urinary bladder symptoms in some patients were found by Straub *et al.* (91) to be related to personal conflicts and affects. Funkenstein (92) reports that there was a correlation of incidence of clinical malaria with the incidence of air raids on Guadalcanal in 1943. From this relationship, it is suggested that anxiety may have been a significant factor in chronic malaria. In a study of tics in children, Mahler (93) notes that various psychopathological processes may be manifested by tics and that the tic syndrome may be comparable to other psychosomatic conditions.

On the whole, the reports in the literature are still at the correlations level. That is, more and more evidence is being accumulated to show that somatic disturbances may be correlated with specific inner psychological processes such as anxiety, hostility, defense mechanisms, and so on. What is lacking so far, however, is evidence on how the effects are mediated. It is certainly a gross oversimplification to assume that the autonomic nervous system is activated by emotion and somehow produces the malfunction and pathology.

Conduct Disorders

A number of general works on delinquency, criminal behavior, sexual psychopathy, and other antisocial behavior have appeared during the year. A collection of papers edited by Eissler (94) presents psychoanalytic discus-

sions of delinquency with particular emphasis on psychogenetics. Tappan's (95) book deals with various aspects of juvenile delinquency. The problem is studied by Sheldon (96) from the point of view of somatotypes. Other books on the criminal include those by Reckless (97), Reiwald (98), and East (99). A 1949 round table discussion on the psychopathic delinquent child [with Karpman (100) as chairman] reflects wide divergence in opinion about the nature and psychological bases of socially unacceptable behavior.

It has been pointed out repeatedly in the literature that antisocial behavior, like neurotic behavior, is symptomatic of an emotional disorder. A few authors are explicit in regarding the delinquent as a particular variety of neurotic. Others disregard the issue, but their writings about the delinquent are remarkably like the reports about neurotics. Typical of this is a report by Abrahamsen (101), who claims that family tension is a basic cause of criminal behavior. Bates (102) amplifies some of Abrahamsen's concepts of the etiology of criminal behavior. He rightly points out that such behavior cannot be attributed to gross and complex family situations that are usually described as the "broken home," "indifferent parents," and so on. What he finds significant in the etiology of criminality is childhood emotional deprivation. Yet this is another term which has become quite ambiguous with repeated use. Here Bates observes that the personality reactions in criminality are comparable to those found in mental disorders; in the offenders, however, the hostility in the family constellation is more intense. The clinician who finds all degrees of tensions and hostilities in the background of nondelinquent neurotic patients can hardly accept Bates' simplified differentiation. Also pointing out that neurotic and delinquent behavior may have similar factors in the genesis, Lampl-De Groot (103) discusses the differences in their derivatives from a dynamic viewpoint.

A few particular types of psychopathic behavior have been discussed. Dayhow (104) reviews a decade of literature on malingering. Aggressive-destructive actions are reported by Gardner (105), lying is evaluated by Karpman (106), and stealing is discussed by Bachet (107). Schmideberg (108) discusses the problem of recidivism.

In keeping with the growing social and legislative interest in sexually deviant behavior, the literature abounds with books on this type of psychopathy. Books in this area include those by De River (109) on the sexual criminal and by Rickles (110) on exhibitionism. Boss (111) and London & Caprio (112) offer dynamic concepts of the psychopathology of sexual perversions. The study of a patient manifesting coprophilia and other sexual deviations is reported by Karpman (113).

Problems of therapy are taken up in a few articles. Schmideberg (114) discusses the treatment of criminals from an analytic viewpoint. A combined approach of individual and group therapy for the psychopath is advocated by Lipton (115). If these methods fail, he suggests the use of electroshock and lobotomy. Unfavorable results of electroshock treatment of several sex psychopaths are reported by Thompson (116). Other discussions include the treatment of prisoners by Mockwood (117) and vocational counseling

in rehabilitation by Fantl *et al.* (118). A rather significant detail in the management of institutionalized psychopaths is brought out by Cruvant & Yochelson (119) in a paper dealing with other aspects of the psychopath. It is noted that such patients tend to generate hostility in the therapist and that this hostility may unwittingly lead to coercive and nontherapeutic attitudes toward the patient. It is noteworthy that as a whole there have been no startling developments in this area during the year.

Similarly, there have been no remarkably new developments on alcoholism during the year. A few general publications are largely reviews and summaries. For example, Seliger (120) outlines the present status of alcoholism as a medical-psychological problem. Goldberg (121) reviews alcohol research in Sweden over a ten-year period. Bowman's (122) brief summary of the field for 1949 confines his remarks to the use of antabuse and group therapy.

It becomes evident from the various case studies reported that although alcoholics have many characteristics in common, the genesis and dynamics are not uniform. A reviewer is led to wonder whether patients differ so widely or whether it is the bias of the investigators. For example, three separate reports bring out such differences. Noble's (123) analysis of an alcoholic woman reveals, among other things, an intense early and cumulative disappointment in the mother and oral fixation. Hall (124) reports obsessive-compulsive features in a chronic alcoholic in whom there are regressive attempts to deal with the conflict of the unresolved Oedipus complex. The third paper, by Newell (125), indicates that her cases, sons of alcoholic fathers, showed a bewildering array of ambivalence and inconsistencies. One of the more thorough discussions of the psychopathology of alcoholism is a paper by Simmel (126), originally published posthumously in 1948 but recently reprinted.

On the basis of some endocrine pathology found in alcoholics, Goldford & Berman (127) suggest that alcoholism might be regarded as a psychosomatic disorder. Papers by Manson (128, 129, 130) and Griffith & Dimmick (131) attempt to characterize the alcoholic in terms of objective psychological test results.

Several types of physiological treatment, none of them new, are advocated. Tentus & Lovell (132) suggest the use of adrenocortical extracts. The conditioned response method is favorably reported by Wallace (133). The use of antabuse, actually another vehicle for conditioned response therapy, is recommended by Glud (134).

Psychotic Disorders

The literature reflects continuation of the longstanding dichotomy in the approaches to the psychoses, the organic and the psychological studies on the etiology and treatment. An address by Terhune (135) expresses the organic viewpoint. He believes that psychodynamic concepts were useful as a temporary expedient but that it is now necessary to correlate and to explain psychopathology on the basis of physiological and biochemical reactions of the nervous system. An excellent annotated bibliography is appended to his

paper. The separate reports on anatomical, chemical, and physiological factors in the psychoses fall short of explaining or correlating with specific behavioral manifestations. However, such studies are still largely in the realm of exploratory basic research, attempting to find areas of measurable chemicophysiological functions which are disturbed in psychotics. For example, there is a renewal of interest in brain cell structure and chemistry of psychotics (136, 137), but the findings are no more definitive than in earlier years.

There have also been continued investigations on metabolic disturbances in psychotics. On the basis of her preliminary studies of carbonic anhydrase, one of the measurable enzymes of the brain, Ashby (138) believes that the faulty distribution of this and other enzymes would account for abnormal mental activity. However, her speculations go further than the present findings warrant. Since the anterior pituitary adrenocorticotrophin hormone has become available, there has been a further interest in the role of the adrenal cortex in response to stresses in schizophrenia. Most of these studies have come from the staff (139 to 144) of the Worcester Foundation for Experimental Biology which has been working systematically on the physiochemical processes in schizophrenia. Various types of physiological stresses were used, and a number of indices of adrenal cortical activity were measured in schizophrenic and control subjects. It is reported that although the schizophrenic patients have functioning adrenal cortices, on the average they are relatively unresponsive to stress. As in previously reported studies, there is an overlapping in the findings between schizophrenic and control groups; not all schizophrenic patients are consistently different from controls. It has justly been said that such investigations have taught us a great deal about physiology but very little about the nature of the schizophrenic disorder. Thus far, there is little to indicate whether the observed physiochemical differences in psychotics are etiologic, results, or concomitants of disturbances of personality functioning.

Despite the trend of increasing clinical emphasis on psychogenic and psychodynamic factors in psychotic disorders, there are relatively few contributions to the literature in this area. Pious (145) offers a theoretical discussion of the pathogenic process in schizophrenia. He postulates that in schizophrenics the super-ego development is faulty because of a pathological relationship with the unconsciously hostile mother during the early months of life. Super-ego functioning remains defective and is unable to exert effective pressure upon the ego to undertake adaptive behavior under stress. Hence a break occurs in the schizophrenic following frustration. The break is manifested by withdrawal and indicates that ego organization functions at a lower level. Rosen (146), referring to schizophrenia, defines psychosis as the state in which imagination becomes reality in order that the individual can survive. Perhaps these are restatements and amplifications of the concept that the psychotic reaction is a defense in which the individual can maintain some psychological integrity.

There have been a few attempts to elucidate the more specific elements in psychotic breakdowns. Warren (147) studied 50 adolescents. Nineteen of

these were psychotics. The report that there was no single etiological factor is not surprising. The early family life of schizophrenics has been considered important for some time and was restudied by Lidz & Lidz (148). They reviewed the case histories of 50 young schizophrenics from the point of view of family relationships, such as deprivation of a parent, instability, and friction between parents. They found that the majority of the cases were "impeded by multiple deleterious influences which were chronically present or frequently recurrent." The paternal influence was harmful as frequently as the maternal. In presenting these data, the authors recognize that such a survey could not provide an understanding of the developmental dynamics which could be gained by the careful study of individual cases. It might also be pointed out that perhaps the really significant and determining elements in early family relationships, such as unconscious hostility of the mother toward the infant, are seldom recorded in case histories.

Another factor in the development of some cases of schizophrenia is reported by Kempf (149). In his view, the conflict over bisexual differentiation causes frustration and guilt which lead to regression. In a study of the role of religion in 68 psychotics, Oates (150) reports that 17 per cent had long-term religious conflicts, that in 10 per cent religion was a precipitating factor, and that in 20 per cent religion provided the ideational content. Rosen & Kiene (151) report on the development of early paranoic reactions in eight cases whose symptoms appeared after sudden promotion to positions of authority. The reactions cleared when they were removed from the situation.

Most of the literature on diagnostic problems refers to the use and interpretation of psychological tests (discussed in another chapter). Difficulties in clinical differentiation between psychoneuroses and early schizophrenia are discussed in a paper by Mace *et al.* (152). Another communication (153) on the same problem points out that a reliable differentiation can be made only after follow-up studies. Thomas & Wilson (154) offer suggestive cues on the detection of pre-schizophrenic states, such as the combination of tension, irritability, and unfounded physical complaints in an individual from a broken home or with an overprotective mother.

The literature for the year offers nothing new in nonpsychological therapy of psychotics. There is some shift in emphasis in electroshock therapy from electrically induced convulsions to the use of electro-narcosis. Clinicians continue to report (155, 156, 157) favorable results from use of these therapies, particularly with depressive reactions. Goldman (158) considers anxiety as the chief indication for electro-narcosis, regardless of the type of syndrome. Comparing deep insulin and electrotherapy for schizophrenia, Rees (159) reports that deep insulin therapy is more effective. There are still no adequate studies on the rationale or *modus operandi* of shock therapies, which must be regarded as empirical.

More extensive use of psychosurgery is reflected in reports of results. There are fewer controversies on the type of technique of frontal lobe operation and more emphasis on evaluating the effects. Hoffman (160), for example, reports that chronic disturbed schizophrenics subjected to prefrontal

leucotomy were improved but not restored to their pre-psychotic level. Similarly, Rosie (161) finds that the operation relieves mental tension and leads to improvement even if the patient does not recover sufficiently for hospital discharge.

Some investigators indicate that it is not enough to know that a patient is relieved of certain psychotic symptoms by severing frontal lobe tracts; there is a need to know what else happens to mental activity. More specific psychological results of the operation are reported by Petrie (162), Robinson *et al.* (163), and Ström-Olsen & Tow (164). Jones (165) studied 24 chronic schizophrenics pre- and postoperatively. He found no organic residuals in the tests after three months and no change in the personality structure. It may be questioned whether the tests and judgments employed in some of the postoperative studies are sufficiently refined. In a summary of psychosurgery for the 1950 International Congress of Psychiatry, Freeman (166) notes that the psychological tests employed "have thrown little light on the profound alterations that have occurred in patients undergoing prefrontal lobotomy."

The original claims that intelligence is not affected by the operation are now seriously questioned. Swartzlander (167) reports that while "global intelligence" is not affected, judgment and social resourcefulness are lowered. Goldstein (168), who has studied the problem extensively, also observes that no characteristic defect of mental functions is revealed by the usual test batteries. But he points out that the actual defect, an impairment in the capacity for abstractions, may be camouflaged by the other results of the operation. The patient becomes abnormally restricted in his desires and abnormally bound to and dependent upon the outer world. Some advocates of this operative treatment attribute the resulting impairment to residuals of the original mental disorder. The problem certainly remains far from solution.

On the whole, there has been no substantial advance in the physical therapies of psychoses during the year.

Psychoneuroses

During the past year, the literature on psychoneurotic reactions has followed the trend of previous years. A book by Burrow (169) presents a general concept of neurosis. Bergler's (170) monograph presents the thesis that fundamentally there is only one basic neurosis, and that all are reducible to a common factor. That is an unconscious masochistic attachment to the earliest image of mother in the pre-oedipal period. Saul (171) expresses another general view that neuroses, and functional psychoses, are reactions involving essentially the same mental mechanisms used by everyone, but to a different degree. A more detailed and specific psychoanalytic discussion of psychodynamics is presented in Pearson's *Emotional Disorders of Children* (172), based on the study of 61 cases.

Other contributions deal with individual dynamic factors, such as Berlin's (173) paper on guilt as an etiological factor in war neuroses. Silverberg

(174) discusses the factor of omnipotence in neurosis. Anxiety and insecurity in relation to neurosis are reviewed by Fischer (175). Harrington (176) reports on a statistical study of family relationships in neurotic patients. It was found that there is generally intrafamily conflict or mental illness in some member of the family. However, the dynamic significance of this is not fully explored. A longstanding impression about the vulnerability of the only child is dispelled by Van Krevelen (177). His studies indicate that although there is a tendency to prolonged dependency, there is no predisposition to psychoneurosis in the only child.

On the basis of electro-neurophysiological investigations allied to cybernetics, Meduna (178) has offered an electronic theory of psychoneurosis. He refers to circuits between the cortex, thalamus, and hypothalamus reverberating as long as any stimuli upset the homeostasis of the nervous system. These reverberations go on until the stimulus is removed, until the organism is removed from the environment, or until the reverberations become intensified to the point of discharge into adjoining pathways. Evidently, by this theory, the neural system having the lowest threshold will receive the excess charge and this will determine the kind of neurosis. Meduna's hypothesis is highly speculative and makes assumptions which are not warranted by our present state of knowledge.

Liddell (179), working with induced abnormal behavior in animals, attempts to relate the dynamics of animal conditioning and neurosis to the problem of anxiety.

A rather new syndrome, refugee neurosis, is analyzed by Pedersen (180), who has examined the problem of psychopathological reactions in extreme social displacement. There are further reports on older subjects. Traumatic war neurosis is reviewed psychoanalytically by Dane (181). Du Bois (182) reports on several cases of compulsion neurosis with cachexia and anorexia. He emphasizes a combination of some constitutional defect plus a basic difficulty in personal relations. In a more comprehensive analytical view of the obsessional person, Lechat (183) points out three cardinal characteristics. These are doubts, scruples, and self-control. Crider (184) discusses the various theories of the nature of phobias. In a clinical report of phobic reactions, Terhune (185) describes the findings in 86 patients. His findings are, perhaps, oversimplified.

There have been some further efforts to evaluate predictions of psychiatric breakdowns in wartime screening procedures. Mayer-Gross *et al.* (186) report the criteria for and results of predicting neuroses in officers of British armed forces. Another phase of the problem is whether soldiers diagnosed as psychoneurotics are necessarily ineffectives. Sharp (187) believed this was not so and reports his experience with mildly psychoneurotic soldiers successfully taken into combat.

No new trends in therapy for psychoneurotics are reported. Bettelheim (188) describes the total therapy for emotionally disturbed children in a special school. Here, where children live in the school, the analytically-

oriented therapeutic program operates through the entire day in the children's relations with one another and with the personnel. The entire environment and the child's living experiences in the school are therapeutic. Representative of physical methods of treatment is the use of insulin reported by Martin (189). His paper emphasizes that the insulin serves as a sedative to allay anxiety; it is considered only an adjunct to other therapy.

Mental Deficiency, Epilepsy, and Organic Brain Disease

Much of the current interest in the field of mental deficiency has centered about the concept of pseudo-feeblemindedness. This is evidently a renewed recognition that many conditions can lower the reaction level of an individual to an apparently feebleminded level in spite of normal intellectual endowment. Arthur (190) discusses some of these conditions, such as physical handicaps, brain injury, severe illness, delayed speech, and intellectual idiosyncrasies. It is noted by Axline (191) also that emotional disturbances may simulate mental deficiency. Other discussions of this aspect are offered by Safian & Harms (192), Fay & Doll (193), and Cassell (194). Jastak (195) presents two main criteria for the diagnosis of feeblemindedness. One is the adjustment level and the other is the IQ score.

There are further favorable reports by Zimmerman *et al.* (196), and Quinn & Durling (197), on the effective treatment of mental defectives with glutamic acid. The importance of recognition and handling of emotional attitudes and ties between parents and child in the clinical management of the defective child is pointed out by Jensen (198). Sarason's (199) book summarizes the problems of mental deficiency.

The literature offers very little that is new about epilepsy. However, there is some effort to describe in psychopathological terms the common observation that emotional states have something to do with seizures. Schick (200) considers the seizure as comparable to an organ neurosis or as a conversion symptom. Pent-up emotions, he postulates, may disrupt the homeostasis in the epileptic brain and thus initiate an attack. In a similar way, Levin (201) attempts to explain the connection between emotional stress and seizures through excitatory-inhibitory mechanisms. Where the excitatory processes prevail, the result is an epileptic seizure, and where the inhibitory processes prevail, the result is a narcoleptic attack. The emotional factors in epilepsy are discussed further by Bridge (202). On the other hand, Lennox (203) disparages the psychological approach to epilepsy, emphasizing that it is a neurophysiological problem. He grants, however, that emotions can precipitate or aggravate, though not originate, epileptic seizures. It appears that these views are not really so far apart; all seem to agree that there is some condition or predisposition in the brain. It is then only a question of what sets off the discharge. Here, Lennox again agrees with the others that emotional states may do so. The differences in emphasis have bearing upon the therapeutic approach. The organicist attempts to raise the brain's threshold to seizures by medication, while the psychotherapist attempts to modify the patient's emotional living.

Behavioral problems associated with speech disorders are discussed by Glasner (204), who reports on the personality characteristics and emotional problems of young stutterers. Johnson (205) notes the frustration experienced in personality and in effective social relationships as a result of speech disturbance.

With respect to the psychological consequences of organic disorders, there is again a dual approach in the literature. Some authorities tend to ascribe the disordered behavior directly to the neuropathology, while others tend to emphasize the role of attitudes and emotional response to the handicap. The first point of view is more traditional and is explicitly expressed in only a few recent papers. For example, Pollack & Thompson (206) report nine cases of mental disorder which seemed to result directly from surgically-treatable lesions. Here, the lesions were not in the brain, but it was supposed that the pathology caused sufficient general metabolic disturbance to impair cerebral function and thus to result in mental disorder. However, there is little to prove that this is the mechanism. The authors add that the mental disorder may have been present in a latent form all along. In the same vein, Thompson & Nielsen (207) discuss paranoid reactions in organic brain disease. Here it is more clearly emphasized that there must be constitutional or personality predisposition to paranoid reactions which are released by some cerebral lesion. Torrens & Otto (208) note that psychosis with multiple sclerosis is rare. Mental changes with amyotrophic lateral sclerosis are described by Friedman & Freedman (209). They suggest that the mental symptoms may be a function of the personality of the individual, not of the disease.

Psychological defects following cerebral injuries are reported in several articles. Speech disorders in cerebral palsy are discussed by Palmer (210). Werner (211) has studied thought disturbances in brain injured children. According to Bakesin (212), disturbances in behavior may be the most prominent manifestation of brain damage in children. He states that the clinical syndrome is fairly characteristic, with subtle neurological and intellectual changes. In all these reports it is implied that the defects are direct results of the brain change. It is noted, however, that Bakesin regards the prognosis as favorable with such emotional support as extra affection, attention, approval, and encouragement. Bychowski (213) discusses some consequences of brain injury from a psychoanalytic viewpoint. For one thing, anxiety is aroused by concern over the damaged part and certain impairments. Symptoms, such as blocking, withdrawals and projections, may then result from the efforts of a weakened ego to protect itself.

With regard to organic brain disease in children, recent emphasis is upon the social and emotional factors. Burton & Jennings (214) discuss these problems in connection with the case studies of six cerebral palsied children. In another study of cerebral palsied children, Little (215) reports the complicating interplay of family reactions to the child. All but one of 22 families showed marked emotional disturbance secondary to having a child with cerebral palsy. There are mixed feelings over the child, including depression,

guilt, overprotection, rejection, and even denial of the condition. The full effect of such attitudes toward the child is not yet fully evaluated. However, Bender (216) elaborates on this problem. She states that the behavior problems of children with organic brain disease can usually be explained by social and emotional conditions in the child's life situation. Frustration and anxiety give rise to problems. She observes that some children with acute encephalopathy who were cherished in adequate homes showed no subsequent behavioral disorders. Thus, the treatment or prevention of disturbed behavior is essentially to meet the child's increased need for human support and to cope with the severe anxiety engendered in the child.

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CLINICAL METHODS: PSYCHODIAGNOSTICS[1]

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It is difficult to discern trends in the recent literature of psychodiagnostics. It is, of course, apparent that interest in projective techniques far overshadows that of any other interest. And among these methods the Rorschach and the Thematic Apperception Test (TAT) continue to maintain their lead. The Rosenzweig Picture-Frustration test appears to be coming into more frequent use, although much controversy centers around basic problems. One of the most encouraging trends is the beginning of experimental attack on assumptions and hypotheses, as in the study of Siipola (1), which have a long history but little objective proof. It is also pleasant to note that interest in validation continues unabated.

Projective Methods

The meaning of the term "projective" when reference is made to projective methods, projective tests, or projective techniques is still the subject of considerable discussion. Frank (2) first proposed the term in 1939. He stated,

> A projective method for the study of personality involves the presentation of a stimulus situation designed or chosen because it will mean to the subject not what the experimenter has arbitrarily decided it ought to mean (as in most psychological experiments using standardized stimuli in order to be "objective") but rather whatever it must mean to the personality who gives it, or imposes upon it, his private idiosyncratic meaning and organization.

The term "projection," on the other hand, was first used by Freud to describe an unconscious mechanism of ego defense. Thus the noun projection and the adjective projective originally did not refer to identical phenomena and the original concept of "projective" is somewhat vague. Recently, Bell (3) has called attention to the distinction between the adjective and the noun but has broadened the meaning of projection to correspond with the adjectival use. He stated,

> The writer would prefer to apply the most common meaning of "projection" stemming from the Latin roots to the use of the word. In this sense it means "to cast forward," which is the action involved in the techniques. The subject manifests his personality in them by "thrusting it out" where it may be inspected. In the "throwing" the personality is not grossly modified; it is only externalized in behavior that is typical of the individual.

As Sargent (4) so aptly pointed out, projection in the Freudian sense may be elicited by the projective methods but "other psychic processes" are also involved. Schachtel (5) also broadens the concept and defines projection "as

[1] This review covers the period from approximately July, 1949 to June, 1950.

that psychic mechanism by which one attributes qualities, feelings, attitudes and strivings of his own to objects (people or things) of his environment." He, too, agrees with Sargent that the defensive process is only one of the processes elicited by projective methods.

Very recently Symonds (6), taking his cue from Bellak (7), classified responses to test stimuli into three groups, the adaptive, the expressive, and the projective. By adaptive he refers to the type of test in which the questions call for an answer and only one answer is correct; whereas

> expressive responses are determined principally by the habit and skill tendencies of the subject. These determinants come largely from within. Perhaps the clearest illustration of an expressive response is a drawing or painting using crayon or brush. Here the response is a product of the subject's imagination, the subject's skill with pencil or brush and his tendency to conceive of his drawing according to such formal criteria as size, position, line, arrangement, color and the like.

He goes on to say that

> projective responses are those in which the subject expresses some fantasy by clothing figures in his stories or drawing with certain attributes or characteristics. The TAT calls forth projective responses par excellence, for in telling a story a subject projects his own trends, impulses, wishes, desires, anxieties and guilts into the characters of his story by identifying with them.

Bellak and Symonds have called attention to important differentiating aspects among projective methods, but it would probably be more confusing than clarifying to split up the projective techniques into the two categories of expressive and projective. Schachtel's definition (5) appears to be the most serviceable at the present time, although further consideration of the concept is necessary.

The Rorschach test.—A number of recent studies have been devoted to the testing of some of the assumptions underlying the Rorschach test. Four studies have been directly concerned with the influence of color and a number of others have touched on the problem indirectly. That color responses are similar in some behavioral respect and differ from responses in other determinant categories was the finding of Wittenborn (8) based on a Chi-square analysis of the Rorschach records of 240 normal and abnormal persons. Support for the assumption that colored blots tend to be "emotionally charged stimuli" was found by Siipola (1). When she presented parts of the Rorschach blots in colored and uncolored versions, she found that the colored blots tended to be both liked and disliked more than the uncolored which tended to be "neutral." Levy (9), on the other hand, found no significant difference between the chromatic and achromatic blots when she used the galvanic skin reflex (GSR) as a measure of affective value. This finding cannot be considered to negate that of Siipola, because of the ambiguous status of the GSR in relation to affect and because she found marked differences in the GSR which were dependent upon the position of the card in her series. A study by Lazarus (10) cast doubt upon the assumed significance of color shock. When he presented colored and uncolored Rorschach slides

to 100 high school seniors, he found (except for the absence of color responses and the possible exceptions of the popular and space responses) no effect resulting from substituting uncolored slides. He also found that the indices used by Beck (11) to determine color shock were unaffected by the absence of colored cards. Roe (12) found that of 20 eminent biologists 17 showed color shock and 16 shading shock. Sappenfield & Baker (13) examined Klopfer's hypothesis (14) that the subject's reaction to color in Rorschach cards VIII, IX and X may cause an underproduction or an overproduction of responses. They applied the regular series of Rorschach slides and a comparable series of achromatic Rorschach slides to 238 college students. Roughly half the number viewed the slides in A, B order while the other half viewed them in B, A order. One week separated the two tests. No differences were found in either the mean percentage of responses to the last three cards or in the variability.

It would appear that the usual significance attributed to color in the concept of color shock has a shaky foundation. It may be, however, that the difficulty in conceptualizing the colored cards, not because of their color but because of their complexity of form is at the basis of the disturbance noted clinically. Siipola's study, however, shows that this is not the complete answer because she found that when the hue did not agree with the form of the blots, responses which could be classified as "color association, farfetched concepts, stereotype and rejection" occurred. A reasonable conclusion from these studies might be that color is not as important as was previously assumed. On the other hand, Siipola's study shows clearly that color does affect responses.

Whether the Rorschach test should be used in such a way as to yield an independent diagnosis is discussed by Ross (15). He makes a plea for considering the Rorschach test not as a diagnostic test but as an aid to clinical diagnosis. He suggests that the major use of the test should be the making of a personality description and warns against using Rorschach data for prognosis or for advising the therapist concerning treatment. It is probable that most clinical psychologists would agree with Ross that the Rorschach test should be used not only in connection with other test data but in connection with clinical data before a diagnosis is made. This would probably be true for most syndromes. The detection of early schizophrenia, however, appears to be a special province of the skilled clinical psychologist who uses a Rorschach test. For example, Mace *et al.* (16) selected from a Veterans Administration (V.A.) hospital 11 cases which had prominent psychoneurotic symptoms but on which the final diagnosis was clearly schizophrenic. It was found that the diagnosis on the basis of the Rorschach test made independently by the psychologist definitely suggested schizophrenia in seven cases. The staff decided, at the diagnostic conference, that only two were schizophrenic, but on further observation in the hospital changed the diagnosis of the others to schizophrenia. In the other four cases neither the Rorschach test nor psychiatric study revealed the schizophrenic process, all agreeing on a diagnosis of psychoneurosis.

The degree to which the clinical psychologist is accurate in using the Rorschach for the diagnosis of schizophrenia appears to be dependent more on the acumen of the psychologist and his interpretation of subtle aspects of the patient's record than on the use of test indications of schizophrenia. Kendig (17), for example, feels that it is significant that, despite 25 years of experience with the Rorschach as a diagnostic tool, test indications of schizophrenia have still not been clearly established. She reviews the reasons for this situation and mentions (*a*) the general lack of clarity as to what constitutes schizophrenia, (*b*) the problem of sub-type classification, (*c*) the fact that many Rorschach studies lump schizophrenics into one category or use differing systems of classification, (*d*) the problem of phase, i.e., the differences between the acute, the chronic, and the deteriorated. She suggests that basically the schizophrenic reflects in his test performance a distortion of reality. The affective psychotic, on the other hand, reveals values which are positively or negatively exaggerated, while the patient suffering from brain disease typically has a blunting in his capacity to deal with the environment.

Although content analysis on the Rorschach test has never been emphasized, it is used on an intuitive basis by many clinical psychologists. Very few articles have appeared in the literature which have subjected the content to systematic study. During the last year, however, at least four articles have been published. Elizur (18) made a content analysis of the Rorschach test in such a way as to yield a measure of anxiety and hostility. Some of the material he presents appears to have possibilities, but there are so many inexplicable features in his work that it is impossible to assess it. For example, the responses "church" and "priest" are scored as anxiety indicators, "pliers" and "teeth" as indications of hostility. Also, the reliability of his measure of anxiety is .48, yet his measure correlates with anxiety ratings based on 45 minute interviews to the extent of .71. A group of college students who volunteered for the experiment has nearly as high a mean anxiety score as a group of neurotic patients.

Pascal *et al.* (19) applied a "testing the limits for sex" procedure to 28 normals, 163 neurotics, and 40 psychotics. After taking the Rorschach in the usual manner, each subject received the cards again and was asked to "point out to me any male or female sex organs that you see." No quantitative differences between the groups were found, but their finding that eight of 14 paranoid psychotics gave anal responses as opposed to one of 26 other psychotics may be of significance. The authors also believe that the technique, when it uncovers responses deviating markedly from those frequently found, is of significance in revealing the dynamics of the case.

Two studies have been made in an attempt to detect homosexuality from the content of the Rorschach, one confining itself to the diagnosis of homosexuality while the other attempted to discriminate between hysterics, homosexuals, and alcoholics. Wheeler (20) utilized 20 types of content responses considered indicative of homosexuality on the Rorschach test in analyzing the records of 100 male patients under treatment in a V.A. mental hygiene

clinic. Thirteen types of responses were found to be suggestive of homosexuality. The extent of agreement between the Rorschach diagnosis of homosexuality and the therapist's judgment was found to be high when the therapist was a psychiatrist (Yule's coefficient of association equals .90), low when a psychologist was the therapist (.28), and absent when a social worker was the therapist (.01). Reitzell (21) studied the Rorschach protocols of 26 hysterics, 26 overt homosexuals, and 26 alcoholics. She examined the content of the responses, using "signs" suggested in the literature as characteristic of each nosological group. For the homosexuals, she found that three of the items used by Wheeler (20) discriminated well but two items did not discriminate. The alcoholics had the least definite Rorschach pattern of the three. In general, the group trends were in the expected direction, but there was so much overlapping that little use can be made of the findings for individual diagnosis.

Schachtel (5) has written a scholarly and ably reasoned critique of the different viewpoints concerning the scoring and interpretation of kinesthetic or movement responses in the Rorschach test, and has advanced his own point of view as well. Although he presents no evidence for his views, his logic is very persuasive. One of his central points, that the character of the specific response is more important in interpretation than the classification into which it falls, e.g., flexor or extensor, is very well taken. Again, although true movement responses are most likely to be found in human figures and in animals in human-like action, animals in animal-like action may at times be scored *M*. Perhaps his weakest point is his attempt to explain why Roe (103) did not find more *M* responses in the artists she studied. It may well be, as he says, that *M* responses do not indicate a capacity for creative production but rather a factor in the capacity for creative experience. One would still expect, however, that painters would have the capacity for creative experience in a rather well-developed degree. It is hard to believe that it is not necessary for an artist to have a creative personality.

Roe (12) found that not only artists but also eminent biologists who were all creative research scientists had few *M* responses. Two had no *M*'s, six had one and three had only two—a total of 11 who had two or fewer *M*'s out of her group of 20. It is striking that there is so little emphasis on *M* in this group. Its explanation is far from clear at present. Mention should also be made of the study of McFate & Orr (22) which presents norms for the Rorschach test for over a hundred normal children based on tests given them at their 11th, 13th, 15th, and 18th birthdays. A compilation of the similarities and differences between the Rorschach records of schizophrenic children, children from foster homes, and children living in an institution, made by Goldfarb (23), is also worthy of note. His findings should be of some help to clinical psychologists who are confronted with the problem of aiding in the diagnosis of schizophrenic children, but since only group findings are presented and there is no cross-validation, the results can be used only as a very general guide. Clinical psychologists who use Beck's Z score (11), designed to reveal the capacity to abstract and generalize, will be interested in learning

that Wilson & Blake (24), apparently ignorant of Hertz' attack on the same problem (25), have simplified the method of scoring. By assigning a weight of 1 to all "organization" responses and counting them, they were able to obtain correlations with Beck's Z to the extent of .98, or higher, in five different normal and abnormal groups.

One of the most important contributions to the literature on the Rorschach test has been made by Cronbach (26) who states, "Perhaps 90 per cent of the conclusions so far published as a result of statistical Rorschach studies are unsubstantiated—not necessarily false, but based on unsound analysis." His review of statistical methods applied to Rorschach scores is not, however, a destructive one as the above statement might imply. Instead, the author appears to be one of the relatively few who are sympathetic to the need for sound statistical methods and at the same time can understand the clinician's point of view. He appears to be equally well informed on methods of statistical analysis and on the special peculiarities of the Rorschach test. His suggestions, which are given in the form of 13 recommendations, should result in a much higher level of statistical analysis for all future workers.

The Thematic Apperception Test.—The Thematic Apperception Test (TAT) as a clinical diagnostic tool has suffered some rude shocks in recent months. Nevitt Sanford says,

> But at the present time it seems that from the point of view of practicality the use of the TAT is something of a luxury; its main—and by itself sufficient—justification lies in its value for research . . . . Now more than ever it stands as an ideal instrument for the systematic investigation of personality. (27)

He goes on to say that the amount of time it takes to administer, score, and interpret is its most serious practical weakness.

Studies by Saxe (28) and by Cox & Sargent (29) indicate other weaknesses, however. Saxe used psychotherapists' judgments as a criterion for his analyses of the TAT. He wrote a brief integrated summary about each patient based entirely on his TAT responses. On the basis of these summaries, he formulated a list of 83 statements covering such topics as relationships with parents, socialization, reaction to anxiety, and ability to form affectional relationships. Each statement applied to one or more of the cases. The experimenter then checked the list, as did the therapists, after they had seen the patients for at least four months. A total of 20 children ranging in age from 9 to 17 were the subjects. On half the patients there was a significant agreement (at the .01 or .02 level of confidence), but only 26 per cent of the items showed agreement at the 5 per cent level or better.

Cox & Sargent (29) administered 10 TAT slides to groups of seventh grade boys in a public school. From these were selected 15 "emotionally stable" and 15 "emotionally disturbed" boys. The two groups were roughly equated for age and socio-economic status, the criteria for stability being no record of treatment and a rating by teachers indicating no need for treatment. The TAT protocols of the 15 "stable" boys were sent to eight clinical

psychologists who had used the TAT clinically and who had done research on it. Each psychologist received two records along with certain information about the cases. He was told that he might have received the records of two disturbed children, two "stable" children or one of each. Eleven of the 15 were judged to be disturbed by the clinical psychologists. The authors suggest that this result may well be due to the absence of normative material, and that disturbed children's protocols are not the obverse of those of normal children.

Rosenzweig (30) also feels that normative material is needed. He says, "It is a remarkable fact that though the Thematic Apperception Test (TAT) has been widely used for over a decade, there have as yet appeared no norms to apply in its scoring or interpretation." Not only do he and Fleming (31) supply apperceptive norms (i.e., responses comparable to "populars" on the Rorschach) based on the responses of 50 men and 50 women for 16 cards, but he also clearly indicates the usefulness as well as the limitations of norms for projective techniques. Rosenzweig (30) justly points out that "to know what should be considered an *individual* response requires that one know just what is *common*. . . . Interpretation begins after such a determination has been made." Care was apparently taken to match the men and women subjects, but it is rather unfortunate that such a high educational and intellectual level was tapped.

It may be also that the nine rules of procedure proposed by Piotrowski (32) will also improve the accuracy of the interpretation of the TAT. In general, his rules make good sense and the reasoning behind them is clear; whether they are correct cannot be judged in the absence of criteria with which to assess them. Many clinicians will probably disagree with his suggestion, "that every figure in the TAT stories expresses some aspect of the testee's personality," thus making it unnecessary to identify the "hero." Concerning the blank card, however, he makes an interesting suggestion which merits further investigation. He suggests that if the story told in response to the blank card does not differ significantly from the rest of the stories, it is likely that the patient has been evasive throughout the test.

It is also likely that clinical psychologists would improve their accuracy if they followed the scoring procedures suggested by Aron (27) in her careful and comprehensive manual for the TAT. It is more likely, however, that Aron's manual will be used for research rather than for diagnostic testing. It is unlikely that clinicians will spend 10 min. per picture in scoring the test, the time which Aron says is the minimum required. If they did, they would find themselves spending from 6 to 7 hr. on the TAT alone, more time than many clinical psychologists have for the entire diagnostic appraisal of a patient. The manual has many excellent qualities—the suggestions for measuring the reliability of scorers being especially noteworthy—but presents very little normative material and validational data.

Weisskopf (33) introduced some interesting variations in the administration of the TAT. She asked 66 undergraduates to write objective descriptions of the 27 TAT pictures in which human figures were used. She cor-

rectly assumed that most subjects would not remain descriptive in their approach. Each picture was scored in terms of the amount of "projective" material given by the subjects, i.e., material which went beyond pure description. The mean number of comments of "projective" character elicited by each picture was called its transcendance index. The assumption was made that pictures which elicited "transcendant" material would be more valuable for diagnostic purposes. She found rather marked differences between the pictures. She also found that pictures in the series for men did not elicit more "transcendant" material in male subjects nor did the pictures in the series for women elicit more of such material from the female subjects. In another investigation, Weisskopf (34) studied the transcendance index under three different conditions. When TAT pictures were reduced in brightness and compared with the usual pictures, no differences in the amount of fantasy material were found. Less fantasy material was elicited as compared with the usual pictures (*a*) when ambiguity was increased by using tracings instead of pictures and (*b*) when the pictures were briefly exposed on a screen.

The Picture-Frustration test.—The Rosenzweig Picture-Frustration (P-F) test has been the subject of six recent papers. Franklin & Brozek (35) criticize the Rosenzweig Picture-Frustration test on the grounds of inadequate rationale, standardization, and validation. They applied the test to 36 men who volunteered for a semistarvation-rehabilitation experiment and found that there were no significant differences in their responses when they were tested after 24 weeks of semistarvation and after 12 weeks of improved diet. They also found that their mean group conformity rating (i.e., "normality") was higher during semistarvation than Rosenzweig's norms based on presumably normal men who were not under any special stress. The authors state that observation showed clear differences between the "starvation" and the "improved diet" conditions in intensity, direction, and type of frustration responses of their subjects. Changes were clearly observed in the "need-persistent" type of reaction. Yet no significant changes were found in the test scores.

On the other hand, Lindzey (36) found a difference significant at the .05 level in extrapunitiveness in a group of 20 college students who were given the P-F test under ordinary conditions and two months later after they had been subjected to strong physical and social frustration. A further finding that extrapunitiveness and intropunitiveness as measured by the P-F test and as measured by four TAT cards did not correspond is not remarkable. A similar but somewhat ambiguous study with somewhat ambiguous results was also made by French (37).

New norms based on 236 males and 224 females from age 20 through 29 are presented by Rosenzweig (38). The group's mean educational level was 13.5 years of schooling. Since insignificant differences were found between this normative group and a group of 110 people from age 30 through 39, the author suggests that the norms be used throughout the twenty-year span. He notes that the new norms, although based on a much larger sample, do not differ markedly from his previous ones. Bernard (39) also found that his norms based on 175 young adult men and women did not differ significantly

from Rosenzweig's original norms. Likewise, Bernard discovered that test-retest correlations (usually based on four-month intervals between tests) were in the neighborhood of from .5 to .75. He found that the group "conformity rating" had a reliability of only .45 and also in another study (40) found that it had little value in the "blind" analyses of the P-F test on normal cases.

Drawing techniques.—The Bender Visual Motor Gestalt test has been the subject of two recent studies. Pascal (41) devised an objective method for scoring the Bender Gestalt test in a way which would distinguish normals from abnormals. In a preliminary report, he demonstrated that his system could be scored reliably (.94) but that test-retest reliability, as measured by 23 normal records after a one-and-one-half-year interval, was only .63. Significant differences were obtained between the scores of normals and neurotics, normals and psychotics, and neurotics and psychotics. Kitay (42) also proposed an objective scoring method for the test. He found a high reliability for his scoring method. However, since he used the split-half technique, which appears to be inappropriate for his purposes, the reliability may actually be lower than it appears.

Studies of patients' drawings have shown a mixed picture. On the one hand, Stonsifer (43), utilizing Goodenough's standards (44), scored the drawings-of-a-man of 39 schizophrenics and 39 nonpsychotic adults roughly matched for age and education. Although the reliability of the scoring was high, there were no significant differences between the means. Royal (45) was able to achieve better results when he attempted to find objective characteristics of drawings of a man and a woman which would differentiate neurotic and non-neurotic adults. His neurotic group was diagnosed as anxiety state or anxiety neurosis and for his normal control he used nonhospitalized dental-clinic volunteers. After cross validation, his original 28 scores were reduced to eight. Using a cutting score, he was able to identify about 47 per cent of the neurotics while misclassifying only about 6 per cent of the normals. Further cross validation would, of course, reduce this separation. Somewhat similar results were obtained by Albee & Hamlin (46) who selected ten cases from the files of a V.A. mental hygiene clinic ranging in degree of adjustment from a "normal" subject to a hallucinated delusional psychotic. Fifteen clinical psychologists were asked to make paired comparison judgments of the patients' adjustment, basing their opinions on the patients' drawings-of-a-man and drawings-of-a-woman. Reliability between two groups of judges was .96 and validity was equal to an r or .64.

In an attempt to develop an instrument which would aid in determining the etiology of mental defectives, Cassel (47) used the Ellis Visual Designs test on 25 endogenous and 25 exogenous mental defectives roughly equated for M.A. and C.A. Applying the test without time limit, he found a significant difference between the groups at the .01 level of confidence. Possibilities for use as a diagnostic instrument are shown by the fact that 96 per cent of the endogenous and only 48 per cent of the exogenous group achieved a score of above five points. Since there is a considerable amount of overlapping

and since the degree of separation achieved may be in part a function of the particular sample of subjects, further research on the problem is called for.

An unusually interesting report was published by Fiedler & Siegel (48) who studied the drawings-of-a-man of 15 improved and 19 unimproved psychoneurotics who had received therapy at a V.A. mental hygiene clinic. By scoring the drawing of the head according to Goodenough's criteria (44), they were able to separate the groups at the .01 level of confidence. Twelve of the 19 unimproved patients could be detected by the use of one cutting score which included only one improved patient. A lower cutting score predicted the failure to improve on the part of 10 patients and included none of the patients who improved.

Other techniques.—Mittelmann, in his foreword to Sonnemann's new book on handwriting analysis (49), states, "Operating as a whole, graphology can take its place as a full equal in personality diagnosis with other projective methods, e.g., the Rorschach test, that have stood the test of time." Most clinical psychologists would, however, not agree. For some reason, American psychologists appear to be more skeptical about handwriting analysis than about other equally complex and "vague" techniques. Perhaps the time has arrived when the claims of graphology should be given a fair hearing. A good start would be to read Bell's chapter on handwriting analysis (3) and then to read Sonnemann's book. Secord's article (50) is well worth reading also, not so much for his negative results, which can be explained by his use of untrained handwriting analysts, but for his critique of methods of validation. A new technique for recording variations in writing pressure, which presumably increases the accuracy of handwriting analysis, is presented by Roman (51).

Other techniques are described by Woltmann (52) who discusses the use of mud and clay, Fischer who describes the World Test (53), and presents some preliminary findings with normal and mentally disordered adults (54) and by Seaton (55) who attempted to devise an incomplete story test with multiple choice endings. Ames & Hellersberg (56) present new normative data for the Horn-Hellersberg "drawing completion" test.

New projective tests are still appearing. A new projective technique with instructions similar to the TAT is proposed by Stone (57). Instead of pictures, he uses sounds, e.g., crying, cheering, barking, thunder, sawing, and "scraps" of dialogue. Sounds are used in sets of three for a total of ten "sound situations." Gehl & Kutash (58) have developed what they call a graphomotor projection technique by means of which "a visible record is secured of the underlying personality." The technique involves seating the subject before a table, giving him a pencil and two sheets of paper, blindfolding him and telling him to let "his hand and mind go freely and to try not to plan to produce anything in particular." After five minutes of drawing or scribbling on each sheet of paper, the subject is asked to tell what he made and what his thoughts were. Then the blindfold is removed, and he is asked to identify any of the structures he has drawn and finally to free associate to the structures identified.

A new kind of masculinity-femininity test, presumably free from culturally determined differences between men and women's interests, was constructed by Franck & Rosen (59). It consists of 36 incomplete simple line drawings which the subject is asked to complete in any way he pleases. Although the omnipresent college student was used for initial standardization, relatively large groups were tested and the authors appear to have made a careful analysis of responses. The potential range of scores is from 0 to 36, yet the mean for men was about 14 and for women, almost 20. Also, the range for men extended from 5 to 24 and for women, from 9 to 28. One might expect that greater separation of the sexes would have been shown, but it may be that in a college population, at least, the obtained differences are correct. The test is not ready for clinical use as yet, but it appears to be a promising instrument. Forer (60) offers a new sentence completion test in two 100-item forms, one for each sex. Items are framed in the first person singular, third person singular, and third person plural on the assumption that impersonal forms would reveal material not possible in the first person. The items are "fairly highly structured for the purpose of forcing the subject to show his hand in those areas of interpersonal behavior in which a diagnostic-therapeutic team is apt to be interested." Systematic coverage of interpersonal relations with father, mother, males, females, people, and persons in authority is provided for. He also provides an extensive check sheet covering attitudes toward and characteristics of people, dominant drives, causes of aggression, anxiety, etc.; reactions to aggression, love, rejection, etc. A new projective test which will undoubtedly be used widely is the Children's Apperception Test recently devised by Bellak & Bellak (61). Consisting of ten pictures depicting animals in various situations, it is designed for children between the ages of 3 and 10. Instructions are then given for administering and interpreting the test, together with illustrations and sample stories taken from 100 records of children from ages 3 to 10. The Thompson TAT (62) may also be very useful in certain situations. Thompson developed a series of pictures comparable to the Murray TAT but using Negro figures instead of white. Believing that Negroes would find it easier to identify with Negroes than with whites, he found support for his hypothesis in that Negroes told significantly longer stories when his pictures were used than when the regular series was employed.

Objective Tests

Personality inventories.—Attempts to improve the Minnesota Multiphasic Personality Inventory (MMPI) are still being made. Brown (63) makes a logical case for dividing the items which the patient places in the "Cannot Say" category into two groups: (*a*) those items which probably do not apply to the patient, and (*b*) those which, at least in some measure, do apply to them. He proposes giving a weight of ½ to each of the latter when it is a scorable item, on the assumption that the diagnostic value of the MMPI will be increased thereby. He presents no data in support of his claim. Although McKinley & Hathaway (104) advise using the complete MMPI with

its 550 cards, a good many clinicians would like to save both their own time and that of their patients by eliminating the 200 unscored items. The question is, however, will such a procedure invalidate the test results? Holzberg & Alessi (64) tried to answer this question by administering a shortened form and the regular form of the MMPI to 30 more or less unselected state hospital patients in A, B and B, A order, with one or two days intervening between the tests. They found *r*'s for the diagnostic scales ranging from .52 to .89, the median being .76. They also found that in half the scales there were statistically significant differences between the mean scores of the two forms. It would appear that somewhat different results are yielded by shortening the test. But the question really remains unanswered, since conceivably the same results might have been obtained if the long form had been administered twice. Results which do not lend themselves to interpretation, but which raise interesting problems, were obtained by Cofer, Judson & Weick (65). They orally administered part of the MMPI to 25 college students while recording the galvanic skin response (GSR). They found no significant differences in the GSR between subgroups who had high and low scores on the inventory. Neither did they find greater deflections on items answered in an "abnormal" direction. Interviews in which inquiry was made on items showing high and low deflections revealed no difference as to "emotional" content.

Humm & Humm (66) disagree with certain conclusions drawn by Ellis & Conrad (67) in their article on the validity of personality inventories in military practice. They feel that it is not necessary to design a personality inventory specifically for the group to which it is applied, that attempting to persuade testees to respond truthfully is naive, that personality inventories are just as effective with sophisticated persons as with less sophisticated ones. Conrad & Ellis (68) reply that although there are many advantages to inventories which are applicable to all sorts of subjects, present evidence appears to point in the direction of the greater effectiveness of specially designed ones. They agree with the Humms that urging honesty is not satisfactory, but point out that the military situation appears to reduce dishonesty. For sophisticated subjects, they suggest that further evidence is needed before the effectiveness of inventories in use at present can be assessed.

The Wechsler-Bellevue scale.—Scatter analysis of the Wechsler-Bellevue scale continues to attract the interest of a large number of psychologists despite, or possibly because of, the fact that "the results of scatter studies are contradictory and confusing" (69). There are many reasons for this state of affairs, not all of which are known by scatter interpreters. Fundamental to all scatter analyses are the reliabilities of the subtests used, yet strangely enough no information was obtainable on these reliabilities until 1949. At this time, Hamister (70) published data based on retesting 53 patients from a V.A. neuropsychiatric hospital. He found two coefficients in the nineties, three in the eighties, four in the seventies, and one each in the sixties and fifties. More recently, Cronbach (71) has published estimates of reliability coefficients for an unrestricted group based on unpublished correlations of

Form I and Form II of the Wechsler-Bellevue. These were obtained by Wechsler by testing 100 patients. They include all subtests except vocabulary. In general, the coefficients are slightly lower than Hamister's, there being one in the nineties, two in the eighties, five in the seventies, and two in the fifties. Although the size of these reliabilities does not rule out scatter analysis, it suggests that positive results are unlikely when using tests in the lower ranges of reliability.

Cronbach has pointed out that the correlation between subtests must also be taken into account. He reproduces Bennett's nomograph (72) and states,

> Applying the nomograph to the difference between Comprehension and Information illustrates its use. The correlation of these two scores, from Wechsler's data, is .69; the reliabilities are .78 and .86. This means that 13 per cent of the differences we find between the two tests are reliable, and that only the largest differences are trustworthy.

There are other factors which must be considered in employing scatter analysis. It is almost always necessary to control age, sex, amount of education, level of IQ, and degree of cooperation of the patient. Furthermore, the practical problems of how to express the scatter pattern are vexing. Should one use as a base for scatter analysis the vocabulary level, the mean verbal level, the mean performance level, the overall mean, or some other combination of subtests? Should scatter be expressed by subtraction or division? Should standard scores or some other type of score be used? Then, too, the reliability and validity of the criterion should be known, but these are often hard to estimate when the criteria are psychiatric diagnoses. When the scores of schizophrenic patients are used, additional factors must be dealt with, i.e., the differential responses of patients in the various subtypes and of patients who are in the acute, chronic, and deteriorated phases of schizophrenia. When so many factors must be taken into account it is small wonder that the findings are "contradictory and confusing."

Recent studies have made contributions at least to the delineation of some of these problems. Jastak (69) has discussed many of them and has suggested means of handling some. Garfield (73) demonstrated that age, IQ, educational attainment, and schizophrenic subtype all affect subtest patterns. Gilhooly (74) reemphasized the relatively low reliability of some of the subtests. Barnett (75) makes a good case for using Z scores instead of weighted scores in scatter analysis, pointing out that for an older age group the mean weighted score in Wechsler's standardization sample was 9.5 for Information but only 5.9 for Digit Symbol. An unfortunate limitation to the use of such Z scores is that the data are not available for converting the Vocabulary weighted score into a Z score. A new base for scatter analysis, originally suggested by Jastak, is proposed by Whiteman (76). It consists in using the mean of the three highest subtest scores. The data used in support of "altitude," as the new measure is called, are not very convincing. Another study by Gilhooly (77) presents fairly convincing evidence that the relation-

ship between variability and ability on the Wechsler-Bellevue is curvilinear rather than linear and small as Wechsler had assumed. If this finding is verified, it will indicate that smaller amounts of deviation from the mean are necessary for significance, the further the IQ deviates from the average in either direction.

Wechsler's hypotheses and statements are still serving as a fertile source for the stimulation of studies. Confirmation of one of his statements is made by Altus & Clark (78). They found that 28 out of 31 Mexican institutionalized juvenile delinquents and 22 out of 22 "white" juvenile delinquents had performance IQ's exceeding those of their verbal IQ's. On the other hand, Hunt (79), using Wechsler's own standardization sample, shows that of the four subtests which Wechsler claims do not decline significantly with age, only two actually hold up well. He suggests that if pathological deterioration is similar to age decline in normal subjects, as Wechsler suggests, a better measure would consist of the two tests which hold up best with age as contrasted with the two that are most affected by it. Jastak (80) calls into question the order of items in the Wechsler-Bellevue subtests. Using the test results of 1,172 cases of children and adults, patients and normal subjects, he found a number of disagreements with the usual order. The greatest disagreement was found on the Picture Completion subtest. Sex differences were sufficiently marked to warrant the suggestion that the sex of the patient be considered in analyses of scatter.

Rapaport's findings (81) are also being checked. Wittenborn (82) examined Rapaport's use of vocabulary scatter patterns on the Wechsler-Bellevue and found that the scatter pattern of any one patient was as likely to differ as much from that of another patient in the same nosological group as it was from a patient with another diagnosis. In checking the scatter patterns, he used small samples selected at random from Rapaport's own cases. His use of rank order of subtests as an index of scatter weakens his conclusion, however, and points up the absence of good statistical tools for dealing with such problems. Rapaport's finding that very few normals and neurotics earn Comprehension scores below the "modified Verbal mean" is confirmed by Gilhooly (74). He did not, however, confirm some of Rapaport's other findings; namely, (*a*) that low scores on Block Design are extremely rare in normals and nondepressed neurotics, (*b*) that a marked drop in Digit Span is a sign of anxiety, and (*c*) that impaired efficiency on the Object Assembly subtest may be a sign of anxiety.

In the important area of the diagnosis of brain damage, most of the results are negative. Andersen (83) found neither the Wechsler index of deterioration nor the Reynell index to be very useful in detecting brain damage in 55 male veterans with verified brain injuries. Rogers (84) also found Wechsler's index a fallible instrument in distinguishing between brain injured patients and neurotics and other groups. In another study (85), he found similar results using Allen's index of deterioration. On the positive side, Hewson (86) developed a new approach to the diagnosis of cerebral pathology. She developed a series of ratios based on different combinations of

Wechsler-Bellevue subtest scores and on a special method of giving and scoring the Woodworth-Wells substitution test. Apparently her original group consisted of only 11 concussion patients, 12 psychoneurotics, and 25 normals. On the basis of empirical findings from these patients, she established the limits for her ratios. Because of several obscurities in the presentation of results, it is possible to estimate only roughly how well her method works on the 118 cases she used for cross validation. It apparently differentiates rather well, for Gutman (87), comparing the Hewson ratios, Wechsler's 12 signs of organic brain damage, and the Reynell index on the test records of 30 carefully diagnosed brain injured patients, finds the Hewson method to be clearly superior to the other two. Although the Hewson ratios are the best, the method yielded 17 per cent false positives and 40 per cent false negatives. The author concludes that the Hewson method is sufficiently helpful to warrant its use together with other psychological tests and clinical procedures. This appears to be a reasonable conclusion at least until further data are available. Another positive finding may be derived from a study by Allen & Krato (88). Their results with respect to the impairment shown by brain injured patients on different subtests are, however, in need of verification.

McNemar (89), after pointing out that eight articles on abbreviated forms of the Wechsler-Bellevue have appeared in the last five years, presents an article which he apparently expects will make further publication on the subject unnecessary. Using Wechsler's original tables as his source of data, he lists the ten best pairs of subtests, as well as the ten best triads, quartets, and quintets. "Best" in this context, means, showing the highest correlation with the full Wechsler-Bellevue IQ. It may be, however, that for some populations, a combination not among his "ten best" may be more highly correlated with the criterion. For example, Cotzin & Gallagher's findings (90) that one of Geil's scales predicted the full IQ's of mental defectives rather well, cannot be ignored. The issue might be resolved if these authors would try McNemar's subtest combinations on their test data.

The recently published Wechsler Intelligence Scale for Children is described by Seashore, Wesman & Doppelt (91). Clinicians will be interested in finding that unlike its parent, the Wechsler-Bellevue, it was standardized on a national sample and that subtest reliabilities and standard errors are given. The scale is generally similar to that of Form II of the adult scale. Arthur (92) has also produced a new resource for clinical psychologists working with children. She modified and restandardized the Leiter International Performance Scale. Using 289 middle class children from three to eight years of age, she presents new norms for this age group.

Miscellaneous

Feifel (93) examined the hypothesis which Yacorzynski (94) advanced to explain the fact that vocabulary test scores of older adults and mentally impaired patients do not decline to any significant degree. Yacorzynski believed that there was a decline in quality evidenced by definitions involving less conceptual organization, but no decline in score because the definitions

given were still granted full credit. Feifel verified this hypothesis in part by comparing 135 normals and 135 psychotics matched on age, education, and score on the Stanford-Binet vocabulary test. He found significant differences between the two groups in quality of definitions. The differences were so small, however, that it is doubtful that they will be helpful in measuring impairment. The fact that he was unable to find clear-cut difference in qualitative types of response between the older and younger normals is also somewhat disquieting.

Another study of impairment or "deterioration" was made by Rappaport & Webb (95). They administered to 10 schizophrenic patients the same group intelligence test which they had taken in junior or senior high school. All but one patient showed a substantial decrement from his previous test but the decrement was highly related ($r = .64$) to a nontest behavioral rating on factors such as attention, concentration, negativism, preoccupation, and apathy. It would appear that the IQ decrement found in many schizophrenics is not due, in large measure at least, to an organic and irreversible impairment.

A symposium of a very interesting kind was held at the Denver meetings of the American Psychological Association (96). One patient had been given 27 different psychological tests. The results of one or two tests were then sent to different clinical psychologists who were asked to "illustrate their methodology and to attempt to arrive at diagnostic pictures of this patient." The interpretations and diagnostic conclusions were read at the meeting and were later published. Few generalizations can be made on the basis of the different reports; perhaps one of the concluding remarks sums it up, "Actual disagreement of results is apparent more often than real." Differences in the clarity of the reports are noticeable, but judgment of the comparative validity of the reports is extremely difficult without (*a*) more adequate criterion data and (*b*) making the assumption that the different tests should reveal exactly the same kinds of data. This assumption cannot be made, of course, but it suggests that if another similar symposium were held, each psychologist should be asked to comment specifically on certain questions. He could then be permitted to say, if necessary, that the tests he was given did not permit answers to some of the questions.

Two studies dealing with the important theme of the clinician have recently been reported. It is generally believed that self-understanding is a prerequisite for dealing with patients in order to prevent the diagnostician or psychotherapist from finding his own unsolved problems reflected, as it were, in those of his patient. Weingarten (97) attempted to test the hypotheses that: (*a*) an individual's perception of other people's problems is directly influenced by his own emotional adjustment; (*b*) an individual sees more problems in those areas in which he is most insecure himself. Results based on a comparison of clinicians' ratings of graduate students' autobiographies with students' analyses of clinical material tended to bear out to a moderate degree the above hypotheses. An attempt to establish the role of self-understanding was not successful and in general the study raised many more questions than it answered. The author did attack an important problem,

however, and it is hoped that more studies of the same and similar themes will be carried out.

Joel (98) calls attention to the fact that the patient just before being tested by the clinical psychologist may feel initial warmth, hostility, or indifference toward him. This attitude may reflect a stable characteristic of the patient or be only temporary, i.e., determined by recent experiences or by his feelings about the anticipated test. The psychologist, too, may have similar feelings toward the patient. Since reactions to each other's attitude presumably affect the patient's responses to projective tests, Joel suggests that the psychologist record his own feelings toward the patient and the feelings he notices or surmises in the patient.

Comments

The development of psychodiagnostic methods is hampered by a number of factors. One major problem is that of criteria for validation. Not only are psychiatric diagnoses frequently unreliable, but often they do not furnish a sufficiently homogeneous group in any one category for validational purposes. When one attempts to use characteristics or traits, however, there is no clear lead from personality theory as to which ones to select. Another problem is the lack of statistical methods suitable for use with the results of projective techniques and for the analysis of patterns. Finally, there is the inadequate use of presently available knowledge of experimental design and statistical methods. Progress is being made with all these problems, however. Although personality theory appears to have a slow rate of advancement, recent progress in statistics and methods of test construction should favor sounder growth in diagnostic testing. For example, the recent books by Goodenough (99) and Cronbach (71), though rather elementary, contain many helpful ideas. Thorndike's volume (100) contains masterly chapters on reliability and validity which should be read by everyone concerned with psychodiagnostic research. Cronbach's article (26) on the Rorschach is also very illuminating. Good suggestions as to methodology are made by Schafer (101), and the symposium led by Zubin (102) constitutes a series of efforts to adapt primarily group-centered statistical treatment to the individual-centered needs of the clinician.

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CLINICAL METHODS: PSYCHOTHERAPY[1]

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It is not only impossible, but probably undesirable that a general review should be written without prejudice. This is more true when the subject is as unstructured as is the field of psychotherapy. Even with great personal insight and restraint, a psychotherapist inevitably carries his own experience into his evaluations. The amorphous character of psychotherapy encourages publication of many articles that are inferior and that make no fresh or different contributions. Omission of, or failure to stress such articles is a duty of the reviewer. But which are the unimportant publications? The problem, of course, is most acute in the borderline contributions. Here significant factual material or the roots of a progressive movement may be underemphasized by the reviewer by his failure to evaluate the article properly. For these reasons, the following statements are offered expressing evaluations and definitions to make more clear the frame of reference within which the review should be considered.

With the intention of arriving at a working definition of the term psychotherapy, at least two extremes in connotation may help in orientation. The first extreme is illustrated by those few psychoanalysts who affirm that there is only one type of psychotherapy, namely, psychoanalytic therapy, and that any other approach is a covering up or ameliorative procedure not properly so considered. Although the most brash proponents of such an extreme concept of psychotherapy are probably psychoanalytic in persuasion, there are here and there other therapists who hold analogously restricted views. At the other extreme, psychotherapy becomes almost any predominantly psychological procedure, personal or environmental, that is assumed to be contributory to mental hygiene or personal adjustment. With this broader definition, better living conditions, better food, and the like, all become psychotherapeutic procedures insofar as they have psychological implications. One might then cite a music program in a clinic or hospital as an example of psychotherapy and the staff musician as a psychotherapist.

Neither of these extreme positions is here accepted as particularly useful for review of the field. The former connotation restricts progress by encouraging doctrinal and isolated positions antipathetic to new ideas. It inhibits active experimentation by persons who might not accept the tenets of the particular procedures defined as psychotherapy. The latter is so broad as to constitute almost a statement of the general principle that simply healthy living is therapeutic. Even the restriction that the word be confined

[1] This review covers approximately the period from April, 1949 to April, 1950.

to procedures specifically instituted by therapists to further mental adjustment or happiness or recovery of patients does not patch up this overly wide position. Some middle road is more useful for these years of transition. The term psychotherapy is here used to refer to all procedures along the continuum from the extreme doctrinal-restricted, position presented above to more non-specific activities which have designedly personal content or form, purposefully prescribed by the therapist to aid persons who are aware that the treatment is for their psychological maladjustment. (This awareness need not be greater than exists with a child, for example, who would have an idea that he is going to the therapist relative to his misbehavior, etc., and who is brought for therapy by a responsible person.) An example of a borderline procedure just falling within the presently accepted definition of psychotherapy would be certain forms of psycho-drama. The staging of written plays would not be considered psychotherapy (although it might possibly be psychotherapeutic). On the other hand, the conducting of a dramatic situation, with patients or staff playing assumed and impromptu parts, presumably related to aspects of the patient's maladjustment would be clearly classifiable as psychotherapy. Another borderline situation can be taken from the field of counseling. It would here be assumed that counseling primarily directed toward vocational adjustment of an adult, or proper placement of a college student in a curriculum, or even within the framework of counseling on problems of social skills would not constitute psychotherapy (although these again would have therapeutic value). Arbitrarily, it is here accepted that there exists a difference between most counseling and psychotherapy in which the clearly psychotherapeutic procedure is illustrated if the counselor finds himself treating specific emotional and maladjustive problems in a client who is asking for such treatment on the basis of accepted personal maladjustment and recognizes that what is going on is in that frame of reference. As further illustration, the psychotherapeutic role of the counselor might, therefore, be initiated by a remark or understanding with the client having the general content, "It seems to me that these difficulties are due in part to some emotional maladjustment of yours. Perhaps we should deal with that for a while before we get back to your counseling problem." The occurrence of psychotherapy is, in this way, tied to acceptance by the patient that his recognized maladjustment is to be treated therapeutically as well as to the consciousness in the therapist that he is undertaking a therapeutic task. General approaches, having adjunctive therapeutic value, but being relatively non-specific and requiring no personal dependent relationship from the client are not psychotherapy.

It may already be apparent that the reviewer is oriented toward the service aspect of psychotherapy—the helping of people by efficient and effective methods. Research or theory having little promise of such an outcome will be considered less of a contribution than those which do. Finally, this reviewer is frankly more favorable to accepting publications that express clear, concise, theoretical summaries or hypotheses or articles that give acceptable statistical data directed toward the above-mentioned service orientation.

There has been too much truth for comfort in comments by persons in other specialties regarding the "talky-talk" quality of much of our literature on psychotherapy.

General Evaluation

One of the more significant advances of the year has been the recognition and extension of therapeutic activities in mental hospitals and other inpatient institutions. Important progress has occurred both in clearly psychotherapeutic services and in auxiliary services such as music training programs and provisions for more cheerful rooms and better food. Shock therapy and brain operation procedures have brought about a revival in the morale of those who must work psychotherapeutically with psychotic patients since remissions can now be produced through these techniques; and while conclusive proof has not yet been forthcoming, it is felt that group therapeutic activities are distinctly beneficial in capitalizing on the remissions. Furthermore, the group work has been extended to the relatives of patients, both to enlist them as active therapeutic aides and to prepare the way for the patient's return home so that his stay may be extended by an appropriate home reception. The revival of therapeutic activity in inpatient settings has been partly mirrored in outpatient adult and child clinics of various types, although apparently the movement is not nearly so vigorous in these settings.

Although much of the initial worry over who is to do psychotherapy has abated, there is still no experimental evidence in the year's literature that would indicate greater contribution from one or another type of training toward the efficient conduct of psychotherapy. The chief contestants in the field, psychiatrists, clinical psychologists, and social case workers, still appear to base their claims upon personal and professional conviction, upon unique elements in their particular training, or upon professional advantage rather than upon experimental comparisons or even really adequate job analyses to show what is effective or required in psychotherapy. Perhaps the failure to do job analyses is very largely dependent upon the failure to develop acceptable hypothetical frameworks in which psychotherapeutic practice is to fit. In this, the psychoanalysts and the nondirective therapists are still unique, both having made vigorous efforts to define the practices of their fields and to express the qualifications of the therapist in terms of definitive practices. Unfortunately, neither of these schools has been able to produce evidence that its methods are better than the catch-as-catch-can (often called eclectic) methods used by the majority of psychotherapists of all persuasions. This year has seen a return to the more normal state in which the arguments among various groups of psychiatrists are nearly as vigorous as are those between psychiatrists and clinical psychologists or any of the other lay pretenders. Especially in the psychoanalytic field, it has become, in the last year, more difficult to determine when one is an analyst. While schisms have always been characteristic of the analytic school, these have become more threatening since publication of the Alexander & French book (4). With it, an ideological approach is provided by acceptably accredited psychoanalysts

whereby nonanalytically trained persons may undertake short term therapy. This book provides an authoritative support to the feeling that real and lasting therapy may be accomplished by brief "eclectic" approaches. Although the authors stress the psychoanalytic foundation of therapy, as they describe the actual procedures, most experienced therapists of all schools can identify much that is like their own methods.

As is usual, by far the greater volume of the year's literature on psychotherapy can be classed under the general heading of descriptive procedure. That is, the authors tell how they applied psychotherapy in some setting and with some individual or group. Probably more articles of this type have appeared on the subject of group therapy than on any of the other subdivisions. There is little evidence of plans to compare objectively the different procedures. Most of the authors simply assert that they feel that their work is good or give testimonials (usually in disguised form) from patients, some of whom will always be willing to support an earnest therapeutic endeavor. If one looks at the contributions on group therapy as a whole, one can hardly escape the feeling that group therapy, like individual therapy, can be effectively conducted in any one of a great many patterns. Each will work, and possibly the requirement for successful group therapy within the limits of our present methods of determining succcess is that the therapist shall have honorable intentions and shall expend a considerable amount of effort in trying to help the patients who make up the group. This reviewer feels that articles merely telling the aspects of therapeutic procedures as seen by the various authors are of little real value. Possibly their most definite contribution is that they create a kind of band wagon effect in which other clinics are encouraged to begin experimentation and to try new approaches. Perhaps this effect justifies the means. Accepting the assumption that psychotherapeutic effort always tends to produce good results, the present movement in group therapy, as well as the continued personal description of how individual therapies are done, will be a good thing in creating confidence in an ever-widening group of persons who will try out such therapy and who may also begin to make real experimental contributions to the overwhelming problem of psychological maladjustment.

Another of the most important movements of this year has been the steady expansion of the field of psychosomatic medicine. Although the immediately postwar expectation that an effective number of physicians, whether in general practice or in a specialty, could become psychotherapists has now been generally modified, there is nevertheless a steady forward progression toward better psychotherapy in medical practice as exemplified in improved presentation of psychosomatic medicine in textbooks available for medical teaching and in some of the journal publications. Some of the journal articles, however, are distressingly poor in scientific structure, and the best contributions to psychosomatic medicine are to be found in the textbooks. It is baffling that even though it is generally accepted that psychological components in the physical complaints of patients are exceedingly important and that psychotherapeutic effort is indicated in a high percentage

of the patients, no clear evidences have appeared of an active attempt to establish practical clinics or to train the number of people needed to handle psychosomatic conditions.

Nondirective counseling continues to provide research publications although no large quantity of material has appeared since the publication of last year's *Annual Review*. It is exceedingly difficult to evaluate the nondirective approach because its positive contribution in drawing attention to research on what is going on in the therapy situation is, to a great extent, counterbalanced by adverse trends in poor treatment of data and circular experimentation. Circular experimentation refers here to the designs in which the therapist says a thing or acts a role and then proves that he has done it; neither the doing nor the proof involve any evidence that the thing in itself is usefully or uniquely related to the external world. What, for example, would be the standing of published data on the interplay of patient-client remarks if the basic methodology being investigated were shown to be psychotherapeutically inefficient? Instead of assuming certain procedures to be good, showing by experiment that we really carry out the procedures, and then, after these steps, circularly implying that the original assumption is proved valid, we should attempt to establish the validity and efficiency of the procedures against outside, uncontaminated criteria of the patient's adjustment. Efficiency of any procedures depends upon the number and kind of patients the procedures improve and on the relative speed and stability with which progress can be achieved.

Probably the most outstanding trend of the year, that having greatest implications for the psychologist, is expressed in papers that directly or indirectly relate psychotherapy to the learning process. The movement in this direction represents the development of a link between the great mass of our traditional psychological knowledge about such subjects as animal learning, perception, and social interaction, on the one hand, and about what is going on in or with the psychologically ill patient on the other hand. The implications are rich in possibility since experimental verification of educational and learning theory and practice, as applied to psychotherapeutic procedure, becomes immediately possible, and our psychological literature provides directly useful constructs that in the next few years may be the focus of attention in the training of psychotherapists. This identification of psychotherapy and learning has aspects of a new movement because, unlike earlier applications of psychological methodology to psychotherapeutic problems, this ties together the two fields in a broader sense. We do not merely accept theoretical or practical data from psychiatry and apply psychological methods of analysis or set up analogical animal behavior, rather, we aggressively identify the therapeutic and psychopathological material of psychiatry with the field of psychology, or at least with that large portion of psychological teaching that bears upon human learning and motivated behavior.

It is intriguing to speculate upon this trend in consideration of the effect it might have upon psychiatric training in particular. At the present time,

neither medical nor psychiatric specialty training includes much emphasis upon traditional psychological subjects. If psychotherapy were to be identified with an educative process in increasing degree, psychology might undergo a kind of renaissance as a basic science to the practice of psychological healing.

In the above contexts the most significant of all the contributions of the year to the field of psychotherapy would, for this review, be the articles by Hilgard (50), Shoben (97), and Shaw (96). These three articles represent recent and timely structural incitants that could, in the next years, trigger off the insurgent research needs of psychologists who are interested in psychotherapy but are nonservice oriented and scientific in their psychological backgrounds. Thus might begin the lacing together of practical psychotherapy and the broad and more basically sound traditional fields of academic psychology.

Research Design and Methods

In speaking of the data presented as exemplary of the research at the Counseling Center, Rogers (81) wrote that two of the most significant results of their research would not be found in any of the studies. "The first of these results is the deepening conviction, 'it can be done—therapy can profitably be investigated by scientific means.'" The second was a change in the locus of security. Admitting that adherence to any clinically effective procedure tends to become dogmatic, Rogers wrote, "But when research is persistently undertaken, the security which all of us must have tends to become lodged, not in the dogma, but in the *process* by which truth is discovered in scientific method." Rogers regards these points as encouragement to therapists of various orientations who are, " . . . hovering timorously on the brink of utilizing research methodology." It is interesting to check the publications that have followed this series of statements to ascertain whether any appreciable number of therapists of any orientation have published data on the process or nature of psychotherapy using acceptable research methodology. Unfortunately, there were no papers. It is heartening, however, to discover that research plans are being executed in several clinics and that a few writers have attempted the description of criteria or methodology.

The most practical paper relating to research design and the general possibility of carrying on research on psychotherapy is that of Hunt (53), who reported methods and progress of a research program conducted in a social agency. The design is founded upon the activities of case workers, and the measures used for evaluation are objective developments of familiar case work terminology. Much emphasis is placed upon the client and his attitude toward the agency. Measurement is in part dependent upon unannounced calls at the home of the client. The paper is of paramount significance because it is a demonstration that research can be done in a setting that many consider to be too service-oriented and too complex. Stycos (102) wrote a much less definitive treatise on methodology and research in mental disorder directed at the problem as seen by the psychiatrist.

Ellis (34) repeated and added to Seeman's five methodological steps (93) to furnish a good statement of a basic pattern for research. Ellis also contributed an outline of what he considers necessary for improvement of psychoanalytic research (35). Du Mas (31), in the last of a series of four papers attempting to resolve some of the vexing problems of objectivity in clinical psychology, treated also the problem of objectivity and evaluation of therapeutic proficiency. He outlined some of the implications that he felt lay behind statistical procedures commonly used and gave partial research designs for the readers' guidance. The points made are excellent, and other papers appearing during the year could have been improved if the authors had heeded the methodological cautions in the articles of Ellis and du Mas.

Miller (71) was critical of points, chiefly in nondirective research reports, when she wrote, "Somewhat prematurely, it would seem, certain theories concerning psychotherapy were considered verified when it was demonstrated that phonographically recorded counselor responses could be classified 'reliably.'" She further emphasized that it did not necessarily follow that counselors' intentions were equivalent to their actual directiveness. Analyzing five interviews, Miller came to this conclusion, among others, "It is apparently possible for nondirective interviews to be as 'directive' as others to the extent that the demonstration of rejection of the client's position can be interpreted as 'guiding' the client toward counselor conceived goals."

With methodology somewhat similar to Hunt's research approach, Axline (7) reported follow-up and other data from case records of children that had been nondirectively handled in therapeutic sessions. The children's attitudes toward the therapy and therapists were particularly investigated to discover how these would be described. It was hoped that the naive views of children might afford evidence for the validity of assumptions concerning the role of the therapist. For the most part, the children did describe their experiences as having nondirective structure. Emphasizing the need for objective data in clinical research, Abt (1) reported methods to analyze what he refers to as "guided clinical interviews." The paper described a system to collect client-attitudes and facts and to reduce these to numerical forms suitable for subsequent research use.

Also on methodology, an article by Schafer (92) occupies a peculiar position, in this reviewer's opinion. Schafer does an admirable job of pointing out some of the assumptions that usually remain implicit in tests and statistics used for clinical research. There is little to criticize in this aspect of Schafer's thinking; the disturbing factor lies in a subtle acceptance by Schafer of methodologies that have a far less rigorous standing than do those he criticizes. For example, he appropriately says that there is no typical schizophrenic record and that we should break down our groups into more homogeneous samples, but then he states that if we are forced to use small samples, we can do so because they " . . . have greater heuristic and clinical potential than the formidable summary scores of huge but inherently chaotic samples." Later, in speaking of seemingly great qualitative differences between two

Rorschach responses that would be scored objectively alike, he says, "A competent interpreter would not be likely, in an individual case, to consider these responses identical in meaning just because their scores are identical." He is here inveighing against the use of scores as adequate representation, but while such a point can be made, we wonder how he would decide who was a competent interpreter. Again, no one could quarrel with him at the practice level, for example, when he says, "Scores are not meant to replace psychological thinking; they are designed to facilitate it, and as such they can be relegated to the background when this is warranted by the logic of the problem." What methods may we use to establish the logic of the problem? There are other examples that might be quoted wherein a similar stand is taken. The critical point here relates to Schafer's implication that the clinician who, admittedly with some justification, has rejected most of the usual objective statistical and classificatory procedures turns to his own judgment or experience. Unless clinical thinking is critical not only of available research methods but also of alternatives to these, we are in danger of continuing the same poor procedures that have so long characterized the field. It may be further illustrative that Schafer, near the end of his article, in suggesting tests for research, speaks not of the established intelligence measurement contribution of the Wechsler-Bellevue but of its debatable patterning applications, and suggests as additional instruments only the Rorschach and the Thematic Apperception Test for scientific personality investigation. When being critical of our usual research procedures, should we not also critically examine the validity of these devices? Perhaps Schafer did not intend to imply that there were now available better approaches than our usual statistics and research practices, but pungent criticism of the one coupled with even benign listing of the other can be read wishfully by the student to relieve him of the need to learn to use rigorously our best (though inadequate) present day methods. While waiting for the development of better statistics and classificatory systems, we cannot consider ourselves free to act in unscientific ways because we can so clearly criticize those presently available.

Several papers reported a little more progress in attempts to measure therapeutically induced changes in the patient. Kauffman & Raimy published an investigation (60) attempting to study the relationship between the Discomfort-Relief Quotient (DRQ) of Dollard & Mower (28) and measures of "self-reference" suggested by Raimy (78), both of which purport to measure changes in self evaluation during counseling interviews. From verbatim protocols to which both measures were applied, the authors concluded that both DRQ and Raimy's measures of "self-reference" (PVAvQ) seem to trace in a similar fashion changes from maladjustment to adjustment. Cofer & Chance (21), also using the DRQ, reported its application to five published nondirectively counseled cases and to one hypnoanalytically treated case. All of these cases were considered successfully counseled and all showed a decrease in the DRQ. Rioch (80) reported the use of the Rorschach in assessment of psychotherapeutic progress, but made only general state-

ments stressing qualitative changes and indicated that statistical analysis would have to await further publication. A series of four papers presented the Goodenough Draw-a-Man test, or modifications of it, as possibly related to diagnosis or other problems in clinical practice. Royal (88) and Stonesifer (101) did not discover useful relationships between the characteristics of drawings and the amount of neuroticism or the diagnosis of schizophrenia. On the other hand, using 10 cases selected for wide differences, Albee & Hamlin (3) found a moderate correlation between factors in drawing human figures and degrees of adjustment ranging from normal to an hallucinated delusional psychotic. Fiedler & Siegel (40) used free drawing as a predictor of nonimprovement in psychotherapy and found that the Goodenough scoring criteria revealed that failure to improve could be predicted by certain aspects of a free drawing test in 12 out of 19 cases.

Without giving experimental data, Neustatter (73) wrote that psychotherapists should be able to indicate the type and length of treatment needed in a given case. He then continued to list various disorders that can or cannot be helped as he evaluated them. Finally, he stressed that revelation of psychogenesis does not effect automatic cures as is often believed, but that it may do so if there is an emotional abreaction (yet not always then), and furthermore that failure to improve should not always be blamed on the patient.

Fiedler (39) reported a very different experimental approach to psychotherapy, using a long compulsory examination as a stress situation predicted to arouse neurotic behavior in college students. The stress situation was preceded by several different types of treatment applied to subgroups of the students. Self-rating scales were used to evaluate the preventive effects of the prestress treatments. Although the reported results were not very convincing, the pattern of research is suggestive of possibilities that might be followed out in other places.

How Psychotherapy Works and What It Is

Although this reviewer found a noticeable scarcity of acceptable research papers on this topic, a few have dealt in reasonably clear terms with descriptions of what the writers believe to be the nature of psychotherapy and the factors that operate in its application. Hilgard (50) laid down a really basic foundation upon which to establish a broad experimental approach to how psychotherapy works and what it is. He presented a concept of the self as a social product in terms that clearly open the way to an experimental program. He told how such a program could be founded upon animal experimentation and recommended the establishment of, " . . . laboratories for the study of psychodynamics fully commensurate with laboratories for the study of perception or learning or other problems of general psychology."

With more special emphases on learning as a basis for neurosis and treatment, Shoben (97) presented a formulation of psychotherapy as a problem in learning theory. He began with an attempt to indicate some of the barriers to psychological research on therapy and then pointed out that there is a

thread of continuity leading to the possibility of scientific study of therapy in the " . . . widespread recognition that psychotherapy is essentially a learning process and should be subject to study as such." He emphasized basic general factors that were accepted by nearly everyone. The first was that " . . . all schools of psychotherapy can with some justice claim cures. . . . " His next three points went a long way toward defining the conditions characterizing a counselor acting as a psychotherapist. He says, "Clinical patients tend to present a primary anxiety as a primary motivation, the goal of the psychotherapist is the modification of the anxiety and all types of counseling employ a therapeutic relationship and conversational content." From this opening Shoben accepted, tentatively, a two-factor theory of learning, and in a preliminary way put the process of psychotherapy into learning theory terms. In summary, he wrote,

> It is here proposed that psychotherapy occurs through three interrelated processes: first, the lifting of repression and development of insight through the symbolic reinstating of the stimuli for anxiety; second, the diminution of anxiety by counterconditioning through the attachment of the stimuli for anxiety to the comfort reaction made to the therapeutic relationship; and third, the process of reeducation through the therapist's helping the patient to formulate rational goals and behavioral methods for attaining them.

Shaw (96) also made a good contribution to psychotherapy in terms of learning theory. He emphasized the role of reward and suggested that the therapist provides reward by making the punishing consequences of neurotic behavior more available to the client. Rewards are available to the patient when, under the protection and guidance of the therapist, the patient begins to establish more rewarding interpersonal relationships outside of the therapeutic situation.

> In summary, a Hullian conceptualization of therapy might be subsumed under these headings: (*a*) maladjustive behavior has immediately rewarding consequences but more remote punishing consequences, as Mowrer and Ullman have suggested, (*b*) this behavior is inhibited through the punishing consequences becoming available to the client, and (*c*) new behavior, replacing the maladjustive behavior, is learned when it is carried out and accompanied by reward.

Magaret (68), in a less complete approach to the same subject, emphasized that in the therapeutic situation the patient acquires a generalized method of learning which permits him to continue his delayed task of socialization. She suggested this as an hypothesis to explain the commonly recognized fact that patients may continue getting better long after the therapeutic sessions have ended. The protocols of Karpman (59) and Carmichael (20) might afford material to test this hypothesis (see below). Hoffmann (51) contributed to the same point in saying, "While it was originally believed that such psychoanalytic insight by itself could provide solutions to neurotic conflicts, it was later recognized that something more than that was necessary, namely, a new and active orientation of the patient." He further wrote, " . . . we must adopt the task of stimulating and activating new factors that will give

rise to processes of healing," emphasizing that most patients need "active instruction" by the psychotherapist. Unfortunately, as Huston & Shakow showed (54), in the schizophrenic we must expect impaired capacity to learn, especially in the early phases of the therapeutic learning process.

Interestingly, as a contribution to theory and practice, Salter (91), described a theoretical pattern for the initiation and perpetuation of a neurosis and for its treatment by psychotherapy. He considers neurotic persons to be "inhibited" in the Pavlovian sense of the term and believes that psychotherapy should properly lead toward "excitatory" behavior. Refreshingly antithetic to the tender approaches of the analysts and nondirective therapists, Salter's book is so actively directive that readers who themselves are a little more inhibitory will find him too irritating to be acceptable. The book cannot be lightly dismissed, however, because Salter is an effective therapist, and his formulations, although certain to be unpopular, are very similar to what really occurs behind the office doors of many psychotherapists even those of some who would be horrified to realize the fact. Incidentally amusing, although somewhat at the expense of careful exposition, Salter inserted numerous succinct epigrams, the most pungent of which are directed at psychoanalysis; typically, "The psychoanalytic interpretation of dreams is poker with everything wild." If a reader is not too repelled by all this excitatory writing, he will find very stimulating material in the book.

Crider (24), like Salter, apparently accepts phobias as conditioned responses, although he emphasized that one type of phobia arises from a conditioned fear whereas the other type arises from a conditioned anxiety. Also referent to phobias, Terhune (107) seems to accept implicitly a conditioning origin of the phobia; also, he certainly considers therapy to be a teaching situation. In his therapy, the patient is taught something of the physiology of the vegetative nervous system, the nature of emotions, and the way in which conditioning of emotional responses can occur. Less closely related to learning theory, but in a similar vein, is the formulation of a theory of anxiety by Ruesch *et al.* (89) in which they stated that anxiety is initiated through concomitant operation of several factors. They stress verbal and visual channels which become cues for detection of anxiety. The emphasis in therapy is apparently on learning proper communication since the process of communication is essential in the management of anxiety. "Alleviation of anxiety through personal contacts is a process which is basic in all interpersonal relations from babyhood to old age." Ross (87), referring also to anxiety, emphasized the causative role of long, continued tensions within the social world of the child. Presumably, these tensions work in an educative way, and treatment of the child cannot be effected without the parent and community being brought into the treatment plan, assumably as a re-educative procedure.

In a still less specific discussion of the process of psychotherapy, Atkin (6) wrote, "One observes the adherents of the three main schools—Freudian, Jungian, Adlerian—working in full faith, eliciting differing psychodynamic explanations from similar types of cases, obtaining not very different per-

centages of cures." He also said that even within any one school, it is difficult to find two therapists that are really doing the same thing. Although these statements were made with reference to difficulties and insecurities facing the psychiatrist in training, they reflect upon the evaluation of the articles reviewed below in that these articles state principles based upon therapeutic successes as seen by the authors but derived mostly from their subjectively analyzed feelings about the effective factors in therapy.

Patterson (74) described what he calls "rational psychotherapy" in which he assumes all functional maladjustments to be alike in basic etiologic factors. The "rational" therapy should be directed at the basic common elements. After an extraneous argument that formal case histories are not essential before undertaking therapy, he compared his ideas with those of Alexander & French (4) developing points of consistency and inconsistency, finally arriving at a confused identification with non-directive methodology.

Sullivan (103) reasserted his previous stand in a timely summary, emphasizing social factors in the psychotherapeutic process. He, like Ruesch *et al.* (89) re-emphasized the assertion that "Therapy works through or is a function of interpersonal communication," pleading for the therapist to give the patient plenty of time to talk.This provides the patient an opportunity to express himself morecompletely and is effective in instructing the therapist as to where he has failed to communicate clearly.

Billings (15) attempted a statement of the dynamics of psychotherapy as practiced by the "eclectic psychiatrist." This ambitious task led inevitably to broad generalizations that probably are faithfully accepted by nearly all psychotherapists not committed to a dogma. One point made by Billings, however, might be controversial. He wrote, " . . . in my experience it is not necessary except in a minority of instances to work toward creating any special type of patient-physician relationship or 'transference' situation in order to accomplish the goal of therapy."

Ziskind, in a significant article (117), considered the problem of specificity in psychotherapy. Starting with what he referred to as " . . . mutually inconsistent schools of psychotherapy each claiming to have a specific cure for mental disorders," he wrote that these cannot all be right, and since complacency and self satisfaction do not stimulate research it is desirable that we examine the extent to which the therapies are specific. Ziskind refers to specific therapy analogously to the use of the term in medicine when specific therapies are chosen to combat an identified etiological factor. In psychotherapy, for example, if one were to accept conflict as the causation of neuroses, then specific removal of conflict would satisfy the requirement for a specific therapy. As the thesis is developed it becomes more and more apparent that very few well supported possibilities for specific psychotherapy are known today. Nearly every therapeutic procedure he considered turned out to be mainly nonspecific. Presumably the nonspecific commonality in various techniques is the explanation for their success in spite of differing hypothetical bases. In summarizing experience in an eclectic clinic he wrote, " . . . recoveries occurred from all approaches despite differences in theory

and technique," indicating that nonspecific elements promoted spontaneous remissions. A second observation from clinical experience was that therapy is usually terminated by the patient rather than by the physician; a third is the impression that what were regarded as basic conflicts in the patient usually are a reflection of the ideologies of the different therapists and their schools. "It was possible to recognize the psychiatrist from the case history." Finally, he says that we should lay more emphasis upon the nonspecific psychotherapeutic procedures and upon current stresses rather than long term character factors or childhood experiences.

As nearly everyone is aware, Rogers (82) emphasizes the necessity for the therapist to rely upon the "strength and potentiality" of the client. Kamm & Wrenn (58) re-emphasize this in saying, "That which the client recognizes as real, not what the counselor sees, is most important." Other authors have emphasized particular qualities necessary in the therapist's attitude or preparation; note particularly the psychoanalytic group. Seeman (94) submitted a short questionnaire to 70 therapists to discover the community of response relative to the relation between therapist adjustment and effectiveness of his therapy. The highest community of responses was found on the subject of making therapy available to the prospective clinician who is to do psychotherapy. Presumably, the motive for making therapy available to the clinician would be to have better adjusted clinicians. However, the 70 clinicians were not in agreement as to whether effective therapists are the best adjusted therapists. The respondents tended to take the position that no real evidence existed on the point. Possibly, with the exception of followers of psychoanalytic and nondirective approaches, the present attitude regarding the role played by the therapist would be like that of Atkin (6). He said that the therapist, " . . . must be taught that psychotherapy remains more an art than a science, and just as there are many styles in art, so there are many types of psychotherapy." Finally he concluded that as the therapist grows older, he will depend more on his sympathy, tolerance, patience, and other personal qualities than on " . . . some rigid theoretical system that he had at one time fondly embraced."

Brief Psychotherapy

Mainly in contrast to the long hours spent with a patient in orthodox psychoanalytic therapy, psychotherapists are becoming more conscious of the advantages of using brief therapy. Of course, brief therapy is not new; most psychotherapy as practiced previously as well as that used today has been essentially brief psychotherapy. On the other hand, there is a marked defensive tendency among certain psychotherapists working in clinics or in other situations where patients have neither time nor money for extended psychoanalytic treatment. This defensive attitude is probably based upon the unproved assumption that longer therapy is automatically better therapy or that psychoanalytic therapy is better therapy, whether long or short. The movement probably has some useful aspects in that it leads to attempts by therapists to tell how they manage to produce results in the relatively

brief treatment. Group therapy, the most widely discussed of the brief procedures, will be reviewed in the next section. These exemplary references indicate the pattern that may be expected in the description of brief psychotherapy.

Salter's book (91) embodies a description of brief methods. One should include the new edition of Diethelm (27), especially since he was one of the first persons to describe a particular method in any detail when he introduced "distributive analysis and synthesis." As an example of a deviant approach, Karpman (59) discussed and illustrated what he called "objective psychotherapy." He had a preliminary interview with the patient, gave him questions in written form, suggested readings, and asked him to write extensive answers to the questions and to make comments on the readings. The therapist then saw the patient frequently, but for brief intervals, and discussed the patient's writings. Following a brief description of the method, there was a detailed protocol of a case so treated. Carmichael (20) also published a protocol of a patient's self written posttherapeutic psychological life. Whitaker *et al.* (115) described a method that combines brief psychotherapy with a procedure for teaching therapists. They have two therapists, the preceptor and trainee, who jointly conduct the entire course of a patient's treatment. Gottschalk (45) was more generally practical in that he provided a complete outline of procedure from selection of patient to termination of the patient-therapist relationship. Kotkov (62) also presented an outline of technique together with some explanatory concepts for brief psychotherapy in an institutional setting. Simon & Chevlin (98) described a program in a Veterans Administration hospital, emphasizing the value of social workers and other personnel contributing to the therapy. Conn (22) described the role of the psychiatrist in the community working with probation officers toward brief psychotherapy of the sex offender. Bartstra (9) and Duchêne (30) both described short psychotherapy with patients, emphasizing its desirability but stressing that there may be dangers and that it is definitely inferior to psychoanalysis, which is the treatment of choice. Grotjahn (46) deplored the emotionalism in the discussion of psychotherapy, particularly directing his arguments to the dispute over brief versus extended psychotherapy.

Group Psychotherapy

There continues to be great interest in the subject of group psychotherapy although the number of publications in this field seems to be decreasing. One writer, Faw (38), has even discussed use of group psychotherapy during the teaching of psychology classes. Wolf (116) described the psychoanalysis of groups, and Rosen & Chasen (84) presented data on a limited experience with a new evolving method whereby psychoanalytically oriented thinking is applied in group sessions with chronic psychotic patients. Loeser *et al.* (66) described group psychotherapy in private practice and contributed practical observations on the method as they have used it. With a somewhat more theoretical approach, Slavson (99) discussed the transference phenomena in

group psychotherapy and emphasized that they are less charged with libido. He described the group dynamics particularly as applied to children, in which case the therapist plays the parental role more realistically than in individual treatment. Kotkov (63) discussed the technique and explanatory concepts applicable within the special frame of reference of the institution. He outlined what happens in group therapy and pointed out that the group provides, " . . . a simplified, less threatening, less competitive atmosphere (e.g., competition is present but it is with sick siblings instead of assumed 'well' siblings) in which members can express similar emotional needs in terms of their conflictual situations and on a relatively equal basis." Joel & Shapiro (55) also described the methods they use in group work and emphasized that while many of the procedures from individual psychotherapy can be extended to group therapy, nevertheless, group therapy is not mass individual therapy and many of its features are unique. Peck (75) presented statistics comparing group therapy as an adjunct with individual psychotherapy used alone. He reported a difference of 15 per cent improvement favoring those who received group treatment as an adjunct. These " . . . tend to develop better insight and adjustment and to change their pathologic attitudes to a greater extent." Mackwood (67) described group therapy in the treatment of offenders in prison; Pfeffer *et al.* (77), group psychotherapy in alcoholics; Blackman (17), in aphasics; and Wender & Stein (114), as an aid to treatment in a psychiatric clinic. Geller (42) described a program of group psychotherapy in the treatment of chronic mental patients. He also emphasized, as do most of the writers, the role of auxiliary personnel in making the group work more effective. All of these found the method useful. Buchmueller & Gildea (19) and Brennan & Steinitz (18) emphasized the value of group therapy with parents. McCarthy (70) made available the transcription of a series of group therapy sessions with alcoholics recorded in an outpatient clinic. This series of papers should be extremely valuable to anyone who wishes to know just what goes on in a group therapy series.

Psychoanalysis

Freud's last book, *An Outline of Psychoanalysis*, was copyrighted as a translation in 1949 (41). It is a general, brief introduction to psychoanalysis and is composed of three parts that are, roughly, a theoretical formulation, a statement of the practical issues, and a consideration of the theoretical implications. Probably the most practical new book on psychoanalysis, however, is the second edition of Glover's book (44). Written for the general medical practitioner, it should have a powerful modern appeal, yet, the approach is reasonably orthodox. Masserman (69) discussed the revisions in analytic therapy and techniques that have appeared. These he considers a natural growth with an optimistic future related to closer cooperation with other fields of psychiatry and ancillary disciplines. In accord with the Chicago school, Masserman suggested that psychiatrists should be able to determine which technique of psychotherapy will be most helpful in each case and use the modifications that seem indicated. Eissler (33) considered

the present state of psychoanalysis as being a crisis. While he is willing candidly to concede that the future may find him wrong, he regards the Chicago school's movement as a break from the better aspects of psychoanalytic theory and practice. He feels that the crisis in psychoanalysis has, until now, been limited mainly to theory but that Alexander & French's book (4) initiated a new phase in which the crisis developed into problems of technique. Rank (79) described adaptation of the psychoanalytic technique for the treatment of young children with atypical development. He emphasized the problem presented by mothers' guilt and anxiety and stressed the importance of creating the right atmosphere in which to proceed with psychoanalytic technique. Many other articles on psychoanalysis appeared during this year which are chiefly devoted to doctrinal discussion or to elaboration of dynamic principles rather than to general description or evaluation.

Psychotherapy with the Psychosomatic and Other Special Cases

Dunbar's publication (32) of a synopsis of the field of psychosomatic conditions should be consulted as a guide book. More clearly a textbook, *Psychosomatic Medicine* by Weiss & English (113), which has two rather good chapters on psychotherapy and much practical material appeared in a second edition. The important role that psychotherapy can play in medical practice was emphasized in briefer articles by Hastings (49) and Van Valkenburg (108) among others. English (36) and Ross & Brown (86) wrote of the role of the psychiatrist in psychosomatic illness. The article by Ross & Brown described the three-step process which they believe to be indicated in psychosomatic practice. The three steps are: integrated diagnostic study, controlled planning, and therapy and follow through.

The best description of a planned rehabilitation service for a special type of patient is the pamphlet by Baker & Brown (8). They prepared in graphic detail a description of a Veterans Administration Hospital program for the rehabilitation of the chronic neurological patient. Data presented indicated that many chronic patients can be made ready for discharge to their homes by a combined physical and psychotherapeutic approach.

Unfortunately, the intent and thinking about psychotherapy for patients having disorders with somatic aspects is much better than the research work or theoretical contributions in the field. There are many very good case reports or reports of treatment of such conditions as ulcers. Among the better papers is that of Almy (5), but there were no articles of an experimental nature. Neither did there appear any articles reporting significant statistics on the special effects of psychotherapy. The cases covered are so few as to be, practically speaking, individual case presentations.

The psychotherapeutic treatment of the alcoholic is discussed by Ryberg (90), who reviewed some of the articles in the field, and by Mueller (72), who listed psychotherapeutic factors in the initial interviewing of alcoholic patients. Klein (61) and McCarthy (70) presented case history material. The latter transcribed a complete series of sessions of group therapy recorded in an out-patient clinic.

As is usual, a large number of papers dealt with psychotherapy for children. These stressed the importance of the child's environment but were, for the most part, not experimental in approach. The article by Buchmueller & Gildea (19) contained a few statistical data relative to group therapy with the mothers of behavior problem children. Rose (83) presented a group of cases in which rejecting parents had placed children of six years or older for adoption. Therapy with these children was not very successful, as was illustrated with three case summaries. Despert (25) summarized records of 250 consecutive cases of children admitted in private practice and made a plea for periodic mental health check-ups. Of therapeutic readiness, she wrote that parental resistance is more common and more powerful than is the child's resistance.

There has been little experimental evidence of progress in psychotherapeutic work with delinquents and other social offenders although there was the usual number of papers. Tarjan (105) emphasized the role of the custodian in the therapeutic handling of the psychopathic delinquent. Conn (22) reported on a liaison service between private psychiatrists and community for psychotherapy of the sex offender, and Adatto (2) and Mackwood (67) described psychotherapeutic approaches appropriate for prison work.

Psychotherapeutic Adjuncts

One of the wholesome modern trends is toward increased emphasis upon adjunctive therapeutic procedures. This is not, of course, a new movement, but the idea had been taken for granted and thereby neglected, at least by many professional psychotherapists who paid lip service to it and never really instituted any programs. Probably the healthiest general approach is to recognize the adjunctive value of the whole milieu whether it is an institution, the home, the school, the neighborhood, or any other environment. Bettelheim & Sylvester (13) emphasized that psychotherapy does not lead to real growth if the child cannot faithfully apply the intermediate or final results of the therapeutic experience to his day life at home. For the institutional approach, Staehelin (100) described the difference in the hospital milieu wherein the hospital had become, not an institution, but a real hospital. He wrote that there is some danger in this greater freedom, but that the farther one goes, the fewer will be the complaints about psychiatric institutions and the greater the benefits to the patients. A more general theoretical approach is attempted by Devereux (26) who started with an assumption of partial sociogenesis of mental disease and in application of this told what can happen by social therapy even on acute wards.

A number of papers deal with special aspects of the environment in relation to psychotherapeutic practice. At least three papers, Pessin & Friedman (76), Taylor (106), and Hulse (52), discussed art as psychotherapy. Bikales (14) and Jones (56) reported the use of psychodrama. Gilman & Paperte (43) spoke of the psychotherapeutic value of music and Van Vleet (109), the value of rhythmic activity as used with children. Lazarsfeld (65) discussed experience with fiction in psychotherapy, but Bellak (10) wrote

with reference to recommending or prescribing texts dealing with mental processes that the reading of such is likely to be positively harmful by adding to the symptoms the patient already has. Even the pen, the telephone, swimming, and dancing come in for a share in psychotherapeutic activities [Watkins (111), Berger & Glueck (12), Kamm (57), and Kurath (64)]. Walker (110) mentioned the intentional production of anxiety in the patient as a sort of adjunctive approach to psychotherapeutic progress. Drake (29) pointed out the similarities of the goals of religion and psychiatry and gave a useful review of relevant literature. Bixler (16) has described limits as defined by nondirective counselors as therapy and has illustrated his meaning by two examples of very aggressive children. Bellak *et al.* (11) suggested the Thematic Apperception Test as a psychotherapeutic adjunct. Both this and the Rorschach are often assumed to provide material that will help to overcome resistance and facilitate the progress of the therapy.

Of a number of papers on the use of hypnosis, those by Conn (23) and Watkins (112) are outstanding. Conn said that hypnosis can be utilized therapeutically for active patient participation as a creative unifying interpersonal experience and can be classified as a form of dynamic psychotherapy. Watkins discussed hypnotherapy of war neuroses and included case studies with considerable verbatim material that may be of value. A number of contributors discussed the use of barbiturates as psychotherapeutic adjuncts. On the whole, the use of narcoanalysis is probably decreasing. This tends to occur whenever a war time procedure is applied to peace time patients who do not show the same dramatic results. It happened with hypnotherapy after the First World War. One note of interest is that patients can fabricate new stories under narcosis and presumptively can keep control of what they say to a much greater extent than has been assumed by some. Adatto (2), for example, made this point in a useful article on narcoanalysis as used with criminal patients.

Training in Psychotherapy

The problem of who should be trained in psychotherapy still occupies a prominent position in the published literature. Last year, however, the number of words devoted to the subject decreased markedly. Possibly the most definitive statement of the position, as between psychology and psychiatry, from the standpoint of the psychiatrist, is to be found in the report of the Committee on Clinical Psychology of the Group for the Advancement of Psychiatry (47). Here psychotherapy was defined as " . . . any procedure, for the management and care of a patient, or the combating of his dissorder by psychological means." Further statements were:

> In conclusion, then, this Committee believes that the clinical psychologist will be able to make his most effective contribution to the total area dealing with mental health and emotional adjustment if he works in direct association with psychiatrists. In such a relationship his maximal effectiveness to persons in need of help will be assured.

The Committee unequivocably opposed the independent private practice of psychotherapy by clinical psychologists and stated that therapy should be

done in a setting where, " . . . adequate psychiatric safeguards are provided." The Committee admitted that the definition of adequate safeguards remains to be achieved. A more balanced statement of the general problem and of relative subject matter can be found in an important article by Shakow (95) in which he presented the issues in a long dialogue between a psychologist and a psychiatrist. Rosenzweig (85) presented some points that bear upon this same subject, saying that confusion was engendered in the present boom of clinical psychology which is characterized by rapid professionalization on the one hand and by the recognition that psychology should concern itself with problems of human nature on the other. A focal point in this would be the conflict between the desire to establish high professional standards with high standards of living as contrasted with the social needs for psychological service that are so extensive that only a very broadly based program could represent proper recognition of the problem. As a solution for all this, Rosenzweig suggested that the Master's degree be the degree qualifying a psychological service worker, and that the Ph. D. be the qualifying degree of psychologists who put increased emphasis on human problems.

Two contrasting articles relating to psychologists doing therapy have to be read to be properly appreciated. These were published by Sward (104) and by Eysenck (37). Sward cited evidence that indicates a fear of psychotherapy on the part of psychologists, and by clear implication, took the position that psychotherapy is exceedingly important as part of the training in clinical psychology. Eysenck, by contrast, speaking from an English point of view, argued that it is neither proper nor desirable for psychologists to spend time on psychotherapeutic training, at least in their professional preparation. It is not proper because psychiatry has prerogatives in the field, and not desirable because therapeutic training is perhaps inimical to good scientific attitudes.

The subject has been discussed in other papers relative to the training of psychologists in psychotherapy and relative to the training of other nonmedical psychotherapists. Considerable confusion would probably be eliminated if the various protagonists spent more time gaining an understanding of the terms they use. The broad definition of psychotherapy, as given in the above mentioned committee report of the Group for the Advancement of Psychiatry (47), for example, seems to this reviewer to make absurd the subsequent restrictions stated by the Committee, since nearly all clinics, whether they are for counseling, employment, guidance, marriage, and the like, would be restricted as would also the fast growing field of religious counseling. For anyone seeking a vigorous statement of the opposing position, two editorials by Harms (48) should be a fair example.

In spite of Seeman's findings (94) that clinicians do not readily agree on the desirability of personal adjustment in the trainees, a number of authors continue to write that the trainee psychiatrist or other psychotherapist should be a very well balanced, tolerant, patient, kindly, etc., etc., sort of person. Atkin (b), an English psychiatrist, went the whole way with this idea in concluding that these fine qualities are what the therapist finally comes to depend on rather than upon some theoretical system.

To this reviewer, all of these varying views seem more likely a function of the lack of clearly stated theories that are general enough to represent properly even the most certain facts from broad clinical experience. Perhaps too few of us really read, or accept as valid, the contributions of therapists who proceed differently from ourselves. If everyone selects his own little group of successful therapeutic examples or his own methods as all the material needed for analysis or hypotheses, then we will continue to have nearly as many procedures and theories as we have writers. If Ziskind's analysis (117), is reasonably correct, all of the special procedures we use have nonspecific therapeutic value—a more adequate theory of therapy will have to be general enough to explain this.

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EDUCATIONAL PSYCHOLOGY

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What amounts to a kind of self-consciousness in educational psychology is indicated in a number of ways. Anderson (1) has discussed the influence of psychology upon education in America within the last fifty years. The content of contemporary texts in educational psychology has been examined by Blair (2). Bruce (3) has raised a number of questions about the amount and kind of training the educational psychologist should receive in general and theoretical psychology. Freeman (4) sees a need of redefinition and reorientation; Trow (5) "charts a course." Noll, Horrocks & Anderson (6) have presented a committee report on the function of the division of educational psychology in the American Psychological Association.

Freeman (4) asks the question, should the teaching of educational psychology be "primarily on a technological level or on a scientific-theoretical level?" Bruce (3) suggests that our efforts to be practical have sometimes resulted in superficial treatment of learning and guidance, and that many who teach educational psychology have had too great a proportion of their training at the practical level and too little in such basic areas as cultural anthropology, clinical psychology, and general psychological theory and experimentation.

To the extent that Freeman's question relates to the training of educational psychologists, Bruce has supplied the answer, at least a good answer. But educational psychology must also justify itself in another respect—the teaching of students who are to become classroom teachers. Most students preparing to teach will not gain enough competency in psychology to make much use of the scientific-theoretical approach, and will not exert a great deal of influence upon educational thought anyway. Perhaps a delineation of broad, general principles of learning, child development and management, and the technology derivable, is the best approach for them.

The serious student of educational psychology, educational specialists, and all persons likely to assume positions of leadership in education should have thorough grounding in the scientific and theoretical aspects of the subject. Principles, generalizations, thought, not technology, can guide us. The educational leaders should be able to distinguish between shallow, unreasoned claims that may be encountered daily in the name of psychology, on the one hand, and principles based on psychological sophistication, on the other.

An educational psychologist is by history any psychologist who devotes his energies to educational problems. Anderson (1) has made a strong case for psychology as an influential factor in education. Dewey and Thorndike "cannot be matched by two others who have had equal effect on American

education." Psychology of personality and mental hygiene has "changed our concepts of what is good classroom control and school discipline," and has "made teachers sensitive to personal-social-emotional problems and needs of children." Olson's (7) recent book is an excellent example of this movement. Also mentioned by Anderson are development of educational statistics and measurements and the resulting knowledge of individual differences, as well as contributions to knowledge of learning and its correlates.

Undoubtedly, one of the pressing needs in our field is a comprehensive learning theory (or behavior theory) that will encompass teaching and learning in school and embrace the broad phenomena of personality and adjustment. The educational psychologist is the logical person to attempt this. Noll *et al.* (6) have made a good start toward a conceptualization of human development as learning. A frank attempt to extend the current learning theory of Hull, Tolman, or others, to learning in school is overdue. After all, the question of whether the theory derived from experiments with rats can be made to explain human behavior is not so crucial as whether it can be made to explain rat behavior. There is not much we can see in a rat that we cannot also see in a person. Most of the theoretical structure rests upon conceptualization. We cannot observe reward or reinforcement, proprioceptive cues, or anticipatory responses, in an animal or a person.

From the standpoint of reinforcement theory, we should have to encompass thought as a form of behavior, which is hardly a bold step. We should have to conceive of it as implicit responding to response-initiated cues. We already have a tradition for this. In connection with reward, we should have to give more attention to self-reward emanating from the person's own motives, purposes, aspirations, and ideals.

We may illustrate some of the possibilities in this approach by reference to habit formation and character. Behavior traits are not acquired, according to reinforcement theory, because they are performed, but because performed and rewarded. They do not sustain themselves out of the dead weight of habit, but because they continue to satisfy motivating conditions—continue to be rewarded. When circumstances change so that they are no longer rewarded, the behavior traits tend to disappear. There is no more striking example than that found in our penal and reform institutions. An inmate in such an institution, or a child at home or in school, may conform because escape from punishment—anxiety reduction—is rewarding, or because by conforming he may gain certain other ends which are rewarding. But when he is no longer under the watchful eye of reform officials or parent or teacher, there is nothing to sustain the behavior traits thus acquired. Little contribution is made to character formation. What a difference it makes when there is acceptance of the aims and purposes of the reform institution, the home, or school—positive identification with them—to the end that their aims and purposes become his own! Then self-reward may sustain the behavior traits. This is the meaning of internal control. As Slavson (73) has said, character is formed by the internalizing of restraints and controls.

We need a guiding, testable, comprehensive learning theory. We need to

see what we can explain by current learning theories. Until some serious attempt is made to extend learning theory to school situations, educators will not have much basis on which to choose from among the various theories; and we will unhappily continue to read that since meaningful arithmetic is more functional than meaningless (stimulus-response) arithmetic, Gestalt psychology is vindicated; or that if a field approach is good enough for physics, it should be good enough for education. Snygg & Combs (8) in their new book have done a better job than most others in discussing educational problems in the light of a particular psychological conceptualization.

School Learning

In reading the journals that afford the usual outlet for research and opinion on learning in school situations, one cannot escape the impression that the accent today is on child growth and development. Academic subjects and skills are not at the focus of educational discussion. They tend to be viewed as a necessary and supporting part of the school child's total development. Then there is a growing realization that academic knowledge and skill may be more efficiently acquired when children have achieved wholesome social and emotional adjustment.

Hildreth (9) sees a good school as one that continues and supplements the adjustive behavior begun in the home; and, if need be, one that can to some extent make up for deficiencies in early home training. Good personal adjustment is a means of preventing failure "due to feelings of insecurity, of resistance, and of apathy in the upper grades." Upon the basis of clinical experience, McCarthy (10) states that the "type of personality adjustment a child makes and the quality of his interpersonal relations in early childhood" have an important bearing upon how well he learns, and that emotional instability is causally associated with most educational difficulties not attributable to mental deficiency. Spache (11), in applying Leckey's theory of self-consistency to educational and therapeutic procedures, advises teachers and therapists to strive to promote a more consistent, integrated "self-concept" [cf. (12)].

How is one to reach a conclusion about the validity of these fundamental ideas, which obviously possess considerable face validity? Gough (13), by an item analysis of the Minnesota Multiphasic Personality Inventory (MMPI), selected 34 items that correlated .43 with honor point ratio (HPR) on a group of high school students and .11 with Otis IQ, utilizing the same population as that on which the analysis was based. On a different high school population, coefficients of .25 and −.02, respectively, were obtained. Otis IQs correlated .62 with HPR. The multiple correlation of Otis IQs and the 34 selected items with HPR was .68. Kahn & Singer (14) found no differences in "adequate adjustment" judgments on the Rorschach test between superior and unsuccessful students, but did find some difference between the two groups on the Minnesota Multiphasic Personality Inventory. Quite likely the basic reasons why unsuccessful students are not doing satisfactory work are of considerable importance in studies of the above-mentioned kinds.

Owens & Johnson (15) report that under-achievers—students not working up to levels normal for their capacities—were characterized by good adjustment and suggest that social orientation and participation may account for underachievement. The reverse is a possibility also. Cronbach (16) obtained low positive correlations (.25 for first year students and .07 for others) between Rorschach adjustment ratings and grade averages of students in the College of the University of Chicago. The multiple R of the Rorschach and the American Council Psychological Examination (ACE) with the grade average was .49 (first year students); r between ACE and grade average for same group was .45. He reports that bright students tend not to earn high grades unless they have at least fair adjustment ratings; and that with low ACE scores, grades tend to fluctuate with adjustment, although, as noted, the trend is not sufficiently marked to have much effect upon the multiple correlation. In such investigations it is a question of what is being validated, the basic assumptions or the instruments used to test them.

Volberding (17) compared successful ($N=34$) and unsuccessful ($N=32$) eleven-year-old pupils on several measures. Some of her differences are as follows: successful pupils, more girls than boys, rated higher in intelligence scores and on socio-economic status, as was to have been expected, but also on the California Test of Personality, Pintner's Aspects of Personality, rating on Friendliness, rating on "Adjustment in the Child World," and liking for competitive games and rough play.

For a decade or more the concomitance between reading disabilities and emotional maladjustment has been attracting wide attention. This relationship has received fresh and extensive treatment and confirmation in reports from the University of Chicago reading clinics (18). From clinical studies one can generalize to other clinical populations of the same kind—in this case a highly selected group intellectually. We should be cautious, however, in attributing like characteristics to poor readers in general. For poor readers of average and superior intelligence, personality deviations may well be causal. Low intelligence plus deficiencies in early training and stimulation and/or unwise school procedures are precipitating causes in the majority of cases of poor reading, and for that matter, scholastic deficiency in general. However, complicating emotional factors commonly associated with failure soon enter the picture and, having done so, may assume a causal role, as the Chicago report indicates. Gellerman (19) observes that reading difficulties are cumulative, becoming more serious the longer they go uncorrected. Lyons (20) has reviewed some of the recent and more competent work on emotional factors in reading, especially as they relate to readiness for beginning reading.

The theory and practice of play therapy certainly are in tune with present-day emphasis in education upon the broader aspects of social, emotional, and intellectual development, as articles by Davis (21) and Axline (22) show. To the extent that diagnoses based upon play therapy sessions are valid, and that reputed improvements in behavior can be verified, play

therapy lends support to the present-day emphasis. But again it is a case of what is being validated. This promising procedure is certainly in need of further validation, both as to diagnostic and therapeutic aspects, against independent criteria. Kubie (23), in a psychiatric look at education, argues for the achievement of emotional maturation through self-knowledge. Group therapy is advocated as a process of socialization and as an aid to the child's understanding of his own thoughts, feelings, and impulses.

Reading.—The period covered by this review has witnessed continued interest in the psychology of school subjects. As in the past, more space is devoted to reading than to any of the other subjects. Interest in emotional and adjustive factors in reading disabilities has already been documented. In the Chicago Clinical Studies (18) additional evidence of the connection between binocular incoordination and reading deficiencies was brought to light. In a review of visual factors in reading for the period 1945-1948, Eames (24) cites a number of papers dealing with physiological and physical factors associated with reading disabilities, particularly factors pertaining to eye movement, lateral dominance, muscular imbalance, illumination, and readability. Krise (25) presents evidence to show that reversals in reading are due to confusion of figure and ground relationships and argues for making pupils more familiar with reversible symbols. Tinker & Paterson (26) investigating speed of reading nine-point type under varying conditions found the best rate with line lengths varying from eight to forty picas and leading of from one to four points.

Yedinack (27) found that children with functional articulatory defects were inferior to a control group both in oral and silent reading. Eames' (28) review of recently published literature on the subject seems to confirm this conclusion.

Various authorities have emphasized the importance of children's experiences prior to entering school. By interviewing parents and pupils, Almy (29) obtained data pertaining to pre-school experience in reading—looking at books and magazines, listening to stories read, interest in letters and words, and opportunities for such experiences. She found low but significant correlations between reported experiences and success in beginning reading for a sampling of first grade pupils representing a narrow range of talent.

In a plea for adjusting reading programs to differences among pupils, Lyons (20) discusses reading readiness in terms of emotional factors, in addition to intellectual and experiential ones. The same point is made by Gans (30). Keliher (31) attributes much of the difficulty to anxieties built up in the child about reading by undue pressures exerted (*a*) by parents, as a disguise for their own insecurity and anxiety, or (*b*) by teachers, as a result of undue demands, starting children before they are ready, or failing to give help at a critical time. These are some of the conditions, she asserts, that have created an enormous demand for remedial reading.

Teachers of reading have long been familiar with work-experience charts: children compose a story orally about some class activity; the teacher writes the story on the board or chart. The children can then read about their own

experience, related in their own words. Familiarity is an aid in reading. The child can check the correctness of his own reading, can determine unfamiliar words from context, and by the whole process should gain a better appreciation of the symbolical function of written words. Carr (32) has made an adaptation of this method through the use of the typewriter in remedial reading. The pupil composes a story which the teacher types as the child speaks. He reports good results with students of average and superior intelligence.

Considerable work has appeared on reading instruction in high school and college. Witty & Brink (33) have analyzed questionnaire data from 109 large high schools. Of these, 97 reported remedial reading programs. This is not to be taken as a typical proportion, since a great many schools in the original sample did not respond. The article describes methods and instructional materials used by responding schools. For example, 27 schools reported using reading-training film; 11, tachistoscopes, etc.

Glock (34) determined for college students the relative effectiveness of three methods of reading training: (*a*) reading a projected film that phrased the subject matter and paced the reader, (*b*) reading from film that paced but did not phrase, (*c*) reading the same text as in films but from printed pamphlets under the directions to read as fast as possible. All three methods produced large gains in reading rate. For the three methods and three different instructors, analysis showed a small overall difference in favor of the first method.

An extensive investigation of the effect of tachistoscopic training in perceptual span and in perceptual speed upon (*a*) reading rate and (*b*) efficiency of subsequent training for speed has been made by Freeburne (35), using college freshmen as subjects. His span group received group training (12 practice sessions) on word and phrase spans ranging from 6 to 34 letters in width, at a constant exposure of 100 ms. The speed group practiced upon 12-letter groups at exposure times varying up to 10 ms. Each group was given 720 separate exposures, plus a pretest and a posttest of 90 items each for span and of 42 items each for perceptual speed. Control groups took pretests and posttests, but did not participate in the practice sessions. Both speed and span experimental groups made significant gains in reading speed; but neither group made significantly greater gain than its corresponding control group. Both experimental groups made significant gains in speed and span respectively, but neither gained significantly more than the corresponding controls. There is a suggestion in this investigation that substantial gains in perceptual speed and span, beyond those accomplished in a few initial exposures, are rather difficult to achieve.

Robinson (36) takes a critical view of methods used in evaluating the results of college reading programs and of methods of selecting subjects for experimental purposes. He argues for evaluation in terms of scholastic performance. Doubtless the use of such a criterion would have a salutary effect of stimulating reading instructors to adopt more rigid standards of instruction.

Weber (37) thinks special disorders and dysfunctions in reading become less important as the adult level is approached. Special disorders have been corrected or pupils possessing them have dropped out of school. He thinks that much of the difficulty in rate comes from poor habits. A skill may "congeal into a fixed habit long before it has realized its full potentialities," and thus trap the individual at an unnecessarily low level. The satisfactions which sustain slow reading habits, he suggests, are the dynamic factors common to all indolence—those for conserving energy and maintaining a "favorable level of . . . inertia" [cf. (38)]

Spelling.—The effect of syllabic presentation of words has been investigated by Horn (39). Syllabic presentation proved to be no better than undivided presentation for any of the five types, such as words difficult to pronounce, or words with double consonant, or prefix and suffix difficulties. Stauffer (40) has published a review of studies of spelling that have appeared since 1945.

Art.—In art education, recent emphasis seems to center upon the relationship between the expressive arts and the broad aspects of child development. Hattwick (41) writes that children's accomplishments with paints or clay or other creative media are not to be evaluated so much by the quality of the final product as by the satisfactions afforded children by the opportunities for self-expression. Hubbard (42) affirms that "self-realization is achieved when one has opportunities to work with materials and create objects." To somewhat the same purpose, Cole (43) says the teacher's role is a psychological one: not to teach in the accepted sense, but to free the child of fear and inhibition and to build confidence in his own way of doing. Ziegfeld (44) has reviewed the recent literature on art education. The trends may be seen in such headings as "Improving Art Instruction," "Art and Personality," "Learning and Creativity in Art," and "Tests and Measurements in Art" [cf. (45)].

Music.—Much the same point of view finds expression in music education literature. Odesser (46) has suggested that music may be so coordinated with projects, experiences, and stories as to foster artistic, social, and educational development. Ehlert & Douglass (47) sees music making as an important contribution to life adjustment and discusses its value for mental health. He argues for a comprehensive program of music education in the public schools.

Arithmetic.—In two articles on meaning in arithmetic, Van Engen (48) discusses meaning of operations or acts performed on objects, social meaning, structural meaning of number, and nihilistic theory of meaning. He defends operational meaning. McSwain (49) offers several practical suggestions to teachers for helping pupils discover meaning in arithmetic. Articles by Coons (50) and Wilkins (51) give several suggestions for motivating pupils in arithmetic work.

Audio-visual instructional materials.—Apparently few important problems in the use of audio-visual materials have been isolated for investigation. Bentley (52) reports a series of experiments on the use of various kinds of audio-visual materials in the teaching of units in high school agriculture.

Their use proved to be of little measurable value. Johnson (53) investigated the use of films and filmstrips in the teaching of two units, circles and loci, in plane geometry. He found these materials to be of some value for the unit on circles [cf. (54)].

Molar experiments of this kind are not likely to be very helpful. Could one say that pictures are not educational? The limitations in experiments involving undifferentiated methods are numerous. There is usually no clear indication that the use of such materials is called for. Instruction may be adequate without them. There is no clear indication of how, where, and what, they are to supplement. Most of us are so poor at test making as to make it unlikely that our tests are sensitive enough to show any material advantages in film presentation were they present.

Ash (55) has investigated the relative effectiveness of massed and spaced film presentation. Since several educational films consist of two or more reels, so that these films may be shown in two, three, or four different class periods, or all in one period, this issue is of some value. Ash found the two methods to be equally effective.

Motivation and work habits.—Krathwohl (56) has derived scores for industriousness in mathematics by comparing data on mathematics achievement with data on mathematics aptitude for a group of entering freshmen. Labeled as industrious were those whose achievement scores exceeded their aptitude scores by a specified amount. Under the reverse conditions students were labeled as indolent. Since these tests were given to entering college freshmen, the scores thus obtained are presumed to indicate how the students applied themselves in high school mathematics. These habits of work formed in high school were demonstrated to "carry over to college algebra" since it was shown that industrious students surpassed normal achievers of comparable aptitude score, and indolent students fell short of the achievement of normal achievers of comparable aptitude score. The over-all difference appears to be significant. It would be interesting to determine how much industriousness scores would add to measures now in use for the prediction of college success [cf. (57)].

An investigation that adds presumptive evidence that Jewish students are more highly motivated than non-Jewish students has been published by Clark (58). He found that Jewish students in the freshman classes at Northwestern University earned higher grades than non-Jewish students of comparable aptitude.

Hearing disorders.—The academic achievements of 28 hard-of-hearing children (9 decibel loss or more) in grades three to seven have been reported by Sprunt & Finger (59). These pupils showed about a half-year retardation on the Stanford Achievement battery in comparison with 28 normal controls of comparable IQ and grade placement.

Some Sociological Correlates

In investigating some of the characteristics of high school seniors who went to college in a midwestern state, Phearman (60) found that the mean

performance of his sample on the Iowa Tests of Educational Development (ITED) stood at the 73.7 percentile on high school senior norms, and that 77.7 per cent exceeded the 50th percentile on the same norms. He found that 96.7 per cent of the boys and 87.5 of the girls in the highest two per cent on the ITED test were enrolled in college in the September following June graduation. Of the top 9 per cent, 68 per cent of the boys and 60 per cent of the girls were also enrolled, as compared with about 27 per cent of all the high school graduates in his sample.

Greene (61) has studied the relationship between the number of grades completed in school, for white and Negro groups in both rural and urban communities, and the following demographic items: family health and comfort, composition of household (e.g., presence of relatives other than children), population mobility (horizontal), and occupational and employment status. In general, significant relationships were found, except for composition of household data. Significantly, a tendency was noted for amount of education to be more closely correlated with the various indices of well-being for whites than for Negroes.

Some difficulties in counseling growing out of race problems are discussed by Williams (62). He finds that misconceptions about the Negro's intellectual abilities, personality, and achievements handicap him in competition for jobs. The difficulties growing out of these opinions are subtly impressed upon a Negro youth by family, peers, and friends, producing feelings of insecurity and anxiety. As a consequence he may repress and ignore interests and aptitudes of which he may be aware, avoid taking a stand that may bring disappointment and conflict later, and manifest indifference to possible opportunities.

Children's concepts and attitudes about American minority and majority groups have been sampled by Radke & Sutherland (63). All the pupils in grades five to twelve ($N = 275$) in a small midwestern city were asked to make free written responses to three questions, "What do you think Americans are like?" and similarly, for Negroes and Jews. (There were no Negroes, and but one childless Jewish family in the city.) Favorable responses about these minority groups decreased, while unfavorable ones increased, from the fifth to the twelfth grade—a condition accounted for by the general culture of the community.

Children's concepts and stereotypes of people of various nationalities, as determined in a certain school district in 1931, are compared by Zeligs (64) with concepts and stereotypes of the same nationalities of children living within the same school district in 1944. He used as a basis various kinds of free responses. Obviously there are some scoring difficulties. For example, why should "Love spaghetti" be scored a favorable trait? We should also like to know if there were any major changes in the character of the population.

Brameld (65) argues for better conceptualization of the field of human relations to the end that it may become an area of fruitful scholarship and of effective action by coping with "misunderstanding, tension, prejudice, conflict," of common interest groups and of whole peoples. He sees as a con-

sequence of better conceptualization the meeting of philosophy, sociology, anthropology, psychology, economics, political science, and international law in a common attack on the problem. Giles (66), in his paper, "Social Change and Schools for Democracy," sees hope in the fact that human beings change, desire to belong, and desire to have affection and recognition. Rockwell (67) sees the task of achieving material improvement in human relationships as a difficult one. In contrast with the comparative ease with which people adopt new tools, he notes the reluctance they display to change in the area of interpersonal and inter-group relations. Here, people may resist change when the advantage in change is clearly indicated. Race prejudice may persist in the face of obvious disadvantages.

Dodson (68) comments upon the relationship that exists between education and social action, as pertaining to group relations. This relationship is seen in three techniques of social action: through involvement, by pressure group techniques, and by legislative and legal action. In appraising human relations at mid-century, Embree (69) observes that concern has shifted from defense of special minority groups to positive action for a common democracy. Instead of a struggle to protect and advance the rights of minority groups, attention is focused today upon the "common advance of all the people" through cooperation.

Boykin (70) analyzes data pertaining to Negro education in 17 states that require complete separation of schools for white and Negro populations. He finds that differences among these states in adequacy of Negro education are as marked and as important as differences in educational provisions between white and Negro groups. The problem of Negro education goes beyond equality of educational opportunity between Southern white and Southern Negro populations. What is required for Negro education in the South is opportunity equal to the average provided in the United States. As desirable as this would be and as great an achievement as it would represent, one must add that unfortunately it could not, without vastly improved economic and social opportunities, usher in the desired reform.

Understanding Children

Perhaps a genuine understanding of children is today the one thing most urgently required in order to establish teaching as a high-level profession. Gallagher (71) emphasizes the inseparability of personality and learning and recommends a wider use of methods of observation in studying the personalities of pupils, particularly in matters relating to self-confidence, motivation, creativeness, and reaction to praise and censure. It seems probable that teachers as a group understand children far better than parents as a group. Still, the typical teacher does not appear to gain much in this respect through years of teaching. If this is true, educational psychology must assume much of the blame. We have not, in teacher-training courses, given teachers the basic training in mental hygiene concepts and practices, nor in the dynamics of personality, required to enable them to make useful observations. One does not make significant observations merely because there is something

that is observable. Much of what goes into observations comes out of one's own head.

Fortunately, a considerable amount of material that is useful in training teachers for a better understanding of children has been appearing within the last few years. Olson's recent book has already been mentioned (7). The studies in human relations as exemplified by Havighurst & Taba's (72) book, by Slavson's (73) earlier contribution, and by Hurlock's *Adolescent Development* (74), as well as by work in play therapy and use of sociometry, are representative of the better materials available. Teachers are becoming increasingly conscious of this kind of orientation, or else the editors of *Childhood Education, Elementary School Journal, Journal of Exceptional Children*, and similar publications have misjudged their audiences. Today, the educational psychologist has his work cut out to interpret the literature of this area to teachers—interpreting it sufficiently well to make a difference in their work.

Davis (21) describes methods of gaining knowledge of inter-personal relationships in a classroom situation and gives some results of therapeutic procedures. Three methods were used to gain information: (*a*) daily observations of signs of social acceptance and rejection; (*b*) analysis of motion pictures of social behavior in free play situations; and (*c*) sociometric analysis. Nine representative children were chosen for play therapy sessions, providing an atmosphere of permissiveness and acceptance. Stiles (75) has described methods of teacher-pupil cooperation in studying children's problems. Cronbach (139) provides a comprehensive discussion of "Observation of behavior."

An interesting commentary is supplied by Axline (22) on children who read too well. We are accustomed to thinking of emotional problems created by retardation in reading. She describes some emotional difficulties arising from reading too much, too soon, and "letting books substitute for friends."

Symonds' paper on implications of fantasy for education (76), as well as his recent book (77), should give the teacher useful insight into the dynamics of child and adolescent behavior, although the projective techniques upon which his research is based are, to say the least, beyond the average teacher. Fantasy "holds the key to the understanding of the meaning of behavior" (76). Understanding children's fantasies will influence the way teachers feel about children, and will be helpful in guidance. Interestingly enough, Ferguson (78) describes her own venture, as a classroom teacher, in a form of projective technique.

The widely-quoted investigation of Wickman (79) comparing teachers' attitudes toward behavior problems of children with those of clinical psychologists has served to emphasize teachers' lack of insight into problems of childhood. On the whole, this investigation has had a salutary effect, despite the fact that the customary interpretations have scarcely been pertinent to the procedure, inasmuch as the two groups were not asked to rate behavior problems on the same basis. A more valid interpretation would be that many behavior characteristics which clinical psychologists regard as serious do not

present serious problems of classroom management; and that some which do are not considered very serious from the clinical point of view.

It is encouraging to observe that many of the articles here reviewed, advancing the mental hygiene point of view in education, have been written by classroom teachers. In the May issue of *Childhood Education* (1950), which is devoted to teacher training, the accent is on training for the understanding of children. Improvement in teacher-training institutions is discussed by Lindsey (80). Efforts of a group of teachers to help themselves are described by Cameron (81). A statewide program of teacher education is described by Weaver (82). Knight & Holdsworth (83) present a program of teacher education through child study.

These articles, as well as many others cited in this chapter, are not based upon research in the sense to which the psychologist is accustomed. They represent teachers' interpretations of good practices from a psychological standpoint. They represent education in practice. It must be agreed that at many points they are practicing our theory. However, no references to the subject of retention and forgetting were seen by the present author while combing the journals in connection with the preparation of this review. Shaffer's (84) excellent review of experimental contributions to mental hygiene should be of some solace to the experimentally-minded reader.

Special Education

Special education usually means education of handicapped children—physically handicapped, mentally retarded, and socially and emotionally maladjusted—on the one hand, and education of the gifted on the other. In some programs speech correction and remedial teaching are included, although these services, being older, more frequently operate under their own organization, both in teacher-training centers and in the public schools. Special provision for the gifted, at least in the form of avowed programs, has not kept pace with provision for the handicapped. This discrepancy may be due in part to the fact that within the last few years we have seen the inauguration of numerous state-wide programs for the education of the handicapped, buttressed usually by special legislative appropriations out of which local schools are reimbursed for excess costs [Martins (85); Graham & Engel (86)]. Similar provisions have not been made for the gifted, although there appears to be an awakening recognition of need of developing leaders in many fields.

Most of our large cities maintain special day schools for the education of physically handicapped children. These schools provide education in the usual sense, plus therapeutic care. Criteria for selection of the child usually embrace the following: (*a*) must be mentally educable, (*b*) must have such physical impairment as to make it difficult or impossible to attend regular school, and (*c*) must have a condition that is remediable by orthopedic care and therapeutic treatment afforded by the schools.

Such schools have developed a great deal of know-how. The progress made with children who, in the beginning, cannot stand or walk, feed themselves

or tie their shoes, is not easily matched in any other area of special education.

Least progress has been made with the mentally handicapped, the area including the largest number of cases. We do not know their possibilities, should the same kind of intelligent work go into efforts to educate them as has gone into the education of the physically handicapped. When mentally handicapped children come into special programs they usually have a history of failure and defeat in regular classes, accompanied probably by feelings of frustration, insecurity, and loss of sense of self-worth. They need emotional and social rehabilitation. They need a program providing opportunities for successful experience, emotional security, self-acceptance, and training in social behavior and often personal hygiene and character formation. The educational possibilities, in the broadest sense, of this group are not known; nor can they be known short of bringing to bear upon the problem the highest educational and psychological skill possible of attainment.

The scope of education of the handicapped may be seen in Dolch's book (87). It treats of the education of children possessing hearing, visual, speech, and health handicaps, as well as those of the crippled, slow learning, feebleminded, emotionally maladjusted, and educationally retarded. Wallin (88) presents a good account of mentally and physically handicapped children and offers some material on their educational and psychological management.

The physically handicapped.—Koenig (89) makes a case for a more objective attitude toward the physically handicapped. A disabled child is isolated socially from his peers to a considerable extent, and is likely to receive deferential treatment from his parents and other adults who play a significant role in his life. He should be treated socially not as a physically handicapped child but as an individual personality. A child's conception of himself must grow very largely out of the reactions of others toward him [cf. Rusk & Taylor (90)].

Cruickshank & Dolphin (91) found that the need of love and affection by orthopedically handicapped children, in their sample, was "over-met," as indicated by performance on Rath's *Self-Portrait N Test.* Paxton (92) describes some of the values in pre-school education for physically handicapped children. She suggests that such training should promote socialization and aid in adjustment to handicaps. Barker (93) calls attention to some psychological handicaps that accompany physical disabilities, such as undue attention from parents and social deprivation. There is also the danger of parental rejection. Cruickshank (94) discusses the impact of physical disability on social adjustment; Cutsforth (95), that of personality crippling through physical disability; and Dreikurs (96), the sociopsychological dynamics of physical disability—in the light of the Adlerian concept.

In occasional surveys over the last 20 years it has been found that physically handicapped children as tested are below normal in intelligence. Since physical handicap is not a medical or psychological entity, general findings without reference to specific handicapping conditions, as among and within categories, are difficult to interpret and without much meaning. Fouracre,

Jann & Martorana (97) present fresh data. For 129 children they obtained a mean IQ of 86.7, distributed by categories as follows: cerebral palsied ($N=65$), 79.3; poliomyelitis ($N=19$), 102.6; congenital muscular anomalies ($N=15$), 93.4; arthritis, tuberculosis of bone, etc. ($N=30$), 90.3. These writers also found a mean educational retardation of 10.1 months in comparison with performance predicted from mental ages, with those in the poliomyelitis category showing the greatest retardation, 17.6 months.

Linck, Shover & Jacobs (98) have treated various aspects of physical handicap, such as examination, diagnosis and selection, classification, characteristics, and curricular and vocational needs of the orthopedically and cardiopathically handicapped. Cruickshank & Peacher (99) have discussed the education of children suffering from epilepsy, tuberculosis, and glandular disorders.

The mentally handicapped.—The Association for New York City Teachers of Special Education (100, 101) has recently published two valuable bulletins on the education of mentally handicapped children. They present "social," "educational," "handicrafts," and "occupational" programs, and discuss curricular, social, occupational, and emotional problems in the light of needs for independence and gratified self-acceptance. Willenberg's (102) description of a "county program of public education for mentally retarded children" is quite instructive. He discusses the legislative, administrative, and budgetary aspects of the program and also the selection and classification of students.

Johnson (103) presents an excellent review of problems connected with the education and guidance of mentally retarded pupils, together with analysis and critical comments. His report presents data on the characteristics of the mentally handicapped. He mentions Harms' (104) finding that they are generally unable to identify with other people, are seldom capable of deep feeling, find change difficult, do best in simple, repetitive situations, and lack vocational adaptability. On the positive side, he cites Kennedy's results (105) to the effect that mentally handicapped workers are almost as fast, as industrious, prompt, and friendly as non-handicapped workers. Johnson (103) recommends that local schools (*a*) analyze the characteristics and abilities of mentally handicapped pupils and (*b*) survey local employment opportunities, and that on the basis of these findings implement a forthright guidance program.

The evidence indicates (103), among other things, that the mentally handicapped (*a*) tend to be less accurate, to be less reliable, and to have poorer judgment than the average worker; (*b*) tend to aspire to occupations beyond their abilities; (*c*) require special training and follow-up supervision; and (*d*) "can and do make good adjustments maritally, socially, economically"; and (*e*) many can perform satisfactorily on semi-skilled and skilled jobs.

Handicapped children have the same emotional needs as other children, only the methods of satisfying them are different, writes Laycock (106). These needs he believes are six in number: affection, belonging, independence,

achievement, recognition, and self-esteem. Outlets are found for these needs in relationships, work, recreation, community service, misbehavior and delinquency, and neurotic traits and illness [cf. (107)].

Kelly & Stevens (108) present a general discussion of special education for the mentally handicapped. Especially valuable is their list of references on research in curriculum and method.

Intellectually gifted.—Practices differ with respect to management of gifted children. Some schools organize special classes for the gifted; others, either from lack of a sufficient number of such pupils to form a special class or from the conviction that the best interest of these pupils is served by educating them in classes made up of a normal cross-section of abilities, do not practice segregation.

It is generally recognized that it is not sufficient to provide special academic work for the gifted. Social development and leadership and training in social responsibility are also emphasized. The better programs give considerable attention to originality, and to imaginative and creative work in connection with the academic side of the program.

There has been some sharing of experience by teachers in the journals. Seegers (109) describes how one school deals with the very bright—IQs above 135—and supplies short case histories. Segregation is not practiced. Brown (110) has related experiences with a group of intellectually gifted children who were kept together as a class under the same teacher through grades seven and eight.

While gifted students have in general been found to be above average in physique, social adjustment, and personality factors, they are, of course, not all so. Strang (111) finds that gifted adolescents may be beset with inferiority feelings and awareness of unsatisfactory human relations. Sumption, Norris & Terman (112) supply an excellent general description of some of the better practices today in the education of gifted children.

Educational Guidance

Several articles outline the function of guidance. Baxter (113) emphasizes prevention as against correction in child guidance. Children should be helped to help themselves in the development of emotional and social strength. Jager (114) suggests that the reason the guidance movement has not met with wider favor is that guidance personnel has taken a too narrow view of their specialty. He sees educational guidance as performing a three-fold function: (*a*) counseling students, (*b*) helping teachers to understand pupils better and to form a broader conception of their functions, and (*c*) supplying data and viewpoint in curricular development. Miner (115) sees in counseling a therapeutic approach to college discipline, as students are led to better understanding of themselves and a better accommodation to restrictions imposed by group life.

Foley (116) writes that educators have a special obligation to help students who do not accommodate themselves to ordinary social pressures to "discover how to know, how to understand, and how to manage themselves."

In an analysis of questionnaire data supplied by counselors and deans, Arnold (117) found that more time is devoted to tardiness, failure, and discipline than to emotional, social, and vocational problems, or to educational planning. Lovejoy (118) discusses the problem of finding colleges to fit the needs of prospective students.

Procedures emphasized in helping an aggressive, socially maladjusted boy to achieve more acceptable adjustment are described by Baker (119). Christiansen (120) recounts her experiences in identifying rejected and ignored children and helping them to a better social adjustment. Stendler (121) calls attention to a number of misconceptions about learning and development that contribute, she believes, to maladjustment. "A child must be corrected the first time he does something wrong," and "he can do anything if he tries hard enough," are examples. Trager & Radke (122) discuss the function of guidance in achieving better human relations among young pupils. They point out the importance, to this end, of parental acceptance of the school program.

Organization of guidance.—Cowley (123) makes a plea for a graduate school of personnel administration, completely staffed. Its faculty should include representatives from all branches of psychology that relate to personnel work, but also to personnel sociology, student demography, and personnel administration. The importance of coordinated work between mental hygienists, counselors, and psychiatrists is discussed by Berdie (124). Abrahamson (125) sees a well-staffed student counseling center as consisting of clinical psychologists, psychiatric social workers, vocational counselors, psychometrists, and an IBM operator.

Guidance techniques.—A brief historical account of guidance techniques is to be found in Wrenn's (126) "The Guidance Movement." Strang (127) discusses basic knowledge in counseling techniques for high school and college workers. The fourth volume of the American Council on Education's Cooperative Study in General Education is devoted to student personnel services as evolved in the cooperating colleges (128). This volume discusses counseling techniques, evaluative procedures, and principles of personnel work.

Cheney (129), in an analysis of high school students' problems as revealed in questionnaires, found that the major problems fell into the following areas: selecting a vocation, marriage, and relationships with people. A survey of practices in meeting pupils' adjustment needs in cities having a population between 50,000 and 300,000 has been reported by Nolan (130). The duties of school psychologists, social workers, and attendance workers are described, as well as the organization of these services.

Paterson's (131) chapter on "Developments in Vocational Counseling Techniques," Bordin's, (132) on "Counseling Points of View, Non-Directive and Others," and Fenlason's (133) on "The Social Worker in the Mental Hygiene Clinic" should prove to be invaluable to anyone interested in acquainting himself with counseling techniques.

Evaluating guidance work.—The problem of evaluating the outcomes of

guidance work is, to say the least, a difficult one. Some idea of the general usefulness of guidance programs may be gained by questionnaire reports from persons who have been recipients of educational counseling or by interview with them, as in Moser's (134) investigation; or by compilation of data relative to social adjustment, participation in extra-curricular activities, and so on, as between students who have received counseling and controls who have not, as in Aldrich's (135) investigation.

In concluding this section, special mention should be made of two books which present contributions of various authorities in this field. They should prove to be indispensable to the student today (136, 137). The recent two-volume compilation of Fryer & Henry (138) contains a number of chapters bearing upon the subject of educational guidance.

Educational Measurement

The year under review has brought forward a number of general reviews and critical treatments of educational measurement that have the effect of consolidating recent developments. Cronbach's book (139) contains an excellent chapter on measures of achievement. In view of the growing interest in broad aspects of pupil development, educational measurement takes in a wider field than formerly. Other chapters in this book, particularly "Observation of Behavior in Normal Situations," including discussion of "Guess Who" and sociometric techniques, are appropriate to the subject at hand. Several chapters in Thorndike's book (140) deal with pertinent problems in test construction. General reviews by Segel & Gerberich (141) on trends in educational and psychological measurement, by Stuit (142) on aptitude tests, and by Findley & Smith (143) on measurement of achievement in school, show recent developments.

These writers reexamine the persistent problems in educational measurement. It appears that standardized tests and statewide testing programs have had a beneficial effect, on the whole. However, without certain safeguards, the results lend themselves to misuse. For example, when teachers, schools, and school systems are compared, teachers may be induced to pass over lightly many of their fine objectives and concentrate on so-called fundamentals. To have general usefulness, tests designed for statewide and national use must confine themselves to elements common to the respective courses of study in all parts of the state or nation. Incidentally, tests of certain basic skills are scarcely subject to this objection, except when teachers and school officials forget that they are tests of basic skills and fail to test achievement in other objectives of their courses. Even at best, there is the danger that school officials may forget to interpret results on standardized tests in the light of local conditions.

Misuse of tests may affect adversely both classroom procedures and curricular development. These matters the authors just mentioned have delineated. The development of better local programs of educational measurement would provide some insurance against abuses resulting from large-scale testing. National and state-wide testing, since it is done on a large

scale and can command the best talent available, can and does provide leadership in the development and refinement of testing procedures. Such programs can actually exert a positive influence on curricular development and improvement of teaching procedures.

Segel & Gerberich (141) have summarized recent trends in educational measurement. They note an increasing tendency to attempt a comprehensive appraisal of pupils, as evidenced particularly in evaluation of local school programs, as well as an increasing use of multiple-aptitude tests, especially in connection with guidance work at the secondary level. Another indication of this trend is the attention currently given to social development and status, as in sociometry, already mentioned, to group dynamics, and even to use of projective techniques, although the problem of validation in the latter is as pressing as ever. They see a third trend in the increasing interest in tests of general educational development, as in the Iowa Tests of Educational Development. Quite as significant is the coordination of guidance and testing programs, as witnessed by the tendency of sponsors of testing programs to provide consultative services. These authors see a need of better rapprochement between test specialists and school personnel, a need that is all the greater in view of the expanding interest in social and adjustive aspects of child development. Obviously these trends represent solid gains. Lorge (144) has presented a paper on history and trends in measurement of achievement, in which is shown the relationship, historically, between testing and curricula. Dearborn (145), in reviewing the relationship between scholastic achievement and socio-economic, cultural, and occupational background, calls attention to the need of considering these factors in counseling and guidance work [cf. (146, 147)].

Several articles have dealt with large-scale testing and evaluation programs. Michaels (148) has surveyed practices in existence in city school systems throughout the country. His investigation covers such items as organization and direction of programs, tests and other evaluative procedures used, types of records kept, etc. He concludes that devices for measuring growth in democratic behavior are in greatest need of improvement. Troyer (149) describes the services offered by the newly-created evaluation center at Syracuse. These, in part, are an assistance to faculty in constructing course examinations and in designing comprehensive graduate examinations. Parenthetically, the tendency to use "evaluation" and "measurement" interchangeably seems unfortunate. Findley (150) discusses the "repressive effects" of large-scale testing programs, such as the New York State Regents Examinations, particularly when local adaptations and interpretations are ignored.

There is evidence of a growing trend toward a more comprehensive approach to prediction of academic success. As already mentioned, Dearborn (145) argues for more attention to students' cultural and socio-economic background as a factor in college success. Even such a matter as socio-economic background should be considered in the light of other factors. One's interpersonal relations with his associates in a cultural stratum is probably a

factor. Havighurst & Taba (72) find that the relationship between socio-economic background and anxiety about scholastic achievement is not linear. Lower class youth tend to have relatively little concern about education. Anxiety increases with successive levels up to but not including the highest level. It is as if one's social and economic position can be so secure that academic success and college graduation are taken lightly. Krathwohl's investigation cited earlier (56) regarding habits of industriousness and indolence holds some promise of usefulness in a more comprehensive approach to the problem of predicting academic achievement.

This trend is further reflected in work with the Rorschach test and other tests of personality and adjustment, as in McCandless' investigation (151) of the Rorschach as a predictor of academic success and the paper of Osborne & Sanders (152) on multiple-choice Rorschach responses of "College Achievers and Non-Achievers." Ostrom's (153) investigation of the feasibility of using the OL score on the Strong Interest Blank as a measure of drive toward scholastic success is another case in point.

One of the better studies in prediction of academic achievement is that of Berdie & Sutter (154) on success of engineering students. They used the Cooperative Survey Test in Mathematics, Cooperative Chemistry Test, USAFI Tests of General Educational Development Test III (Interpretation of Reading Materials in the Natural Sciences), high school rank, and other measures. For three different groups of students, R's were obtained between these tests and HPR's earned in first quarter's work as follows: .79, .66 and .66. The best single predictor was found to be high school rank, although the GED Test III proved to be nearly as good. This investigation in prediction appears to be in line with Stuit's observation (142) that the trend is away from subject matter aptitude tests and toward measures of previous achievement.

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COUNSELING METHODS: DIAGNOSTICS[1]

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As pointed out by Berdie (1), the need for diagnosis in counseling has been the subject of considerable discussion in recent years. During the early period of the development of technical counseling facilities in colleges and secondary schools, a great deal of emphasis was placed upon diagnosis. Occasionally a voice was raised, questioning whether a client was really helped very much by the process of informing him of his strengths, weaknesses, interests, and adjustment mechanisms; nevertheless, diagnosis continued to occupy a prominent part in the total counseling process. Rogers (2) and his students constituted the first group or school of thought who raised persistent questions about the usefulness of diagnosis in counseling, particularly the making of a diagnosis as a sort of end in itself. These questions raised by Rogers and his students have served to place diagnosis in better perspective and have stimulated more serious study of the role of diagnosis in the counseling process. However, despite these questions, and granting their significant contribution, it can hardly be denied that modern counseling procedures owe much to the development and use of diagnostic instruments.

Williamson, in the preface to *Predicting Success in Professional Schools* (3), has recently evaluated the role of diagnosis in counseling in the following manner.

> The effectiveness of educational and vocational counseling has increased as a result of improved accuracy of diagnoses which are based upon the use of scientifically derived and constructed instruments. This conclusion does not argue that the whole of counseling is identified with, and restricted to, scientific technology. But it is rather to argue that any type of remedial counseling which is not based upon accuracy of understanding of the condition of the client has limited chances of being effective. One exception to this generalization may be found in certain types of therapy in which diagnosis is of less importance and self-understanding achieved by the client himself is the all-important aspect of perception of relationships among causes and symptoms.
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> But in educational and vocational counseling, accuracy of diagnosis of aptitudes, interests, and achievement is the most important fundamental of effective counseling. It is in this field that the scientific movement has made its most noteworthy contribution to effective counseling. The development of standardized yardsticks in the form of psychological tests needs no defense, although they need continuous exposition and description.

It is, in other words, not a question as to whether diagnosis shall be used in counseling. It is rather a question of what techniques should be used,

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when they shall be applied, and how they shall be interpreted. The answer to this question depends upon continouus research and careful evaluation of present counseling procedures.

General Summaries and Surveys

A comprehensive summary of research pertaining to the prediction of success in professional schools was prepared by a sub-committee of the Committee on Student Personnel Work of the American Council on Education (3). The first chapter deals with the general problem of prediction and discusses in particular the role of prediction in counseling. Succeeding chapters deal with the prediction of success in Engineering, Law, Medicine, Dentistry, Music, Agricultural Training, Teacher Training, and Nursing Schools. Each chapter includes a bibliography and a discussion of the following topics: Description of Training Program, Requirements for Admission to Training, Review of Research Findings, and Implications for Counseling.

Of special interest to those concerned with diagnosis in counseling are the tables of correlation coefficients showing the relationship between various predictive indices and criteria of success and the summary tables showing the essential qualifications believed to be required in each field and the recommended predictive tests and standards. The section "Implications for Counseling," included in each chapter, is largely concerned with diagnosis and prediction. The Committee, in assembling this large volume of research data, has made the results of research pertaining to the prediction of success in professional schools more readily available to counselors and has pointed out particularly the deficiencies in present knowledge concerning diagnosis and prediction. The report should, therefore, serve a useful purpose in summarizing present knowledge and as a stimulus for further research.

Buros' new yearbook (4) and Super's volume (5) continue to be the most helpful general source books for the counselor in evaluating tests and providing information regarding limitations of the tests. Cronbach (6) has written an excellent introductory textbook stressing the general principles of testing with the aim of acquainting the student with techniques which will be helpful in the selection of appropriate tests for a particular need. Evaluative material is presented on some representative, widely used tests in various areas. Cronbach has divided the text into three parts: basic concepts, tests of ability, and testing of typical performance. The book constitutes an exemplary job of integrating the practical and clinical aspects of testing with the theoretical and statistical approaches to testing problems.

Several introductory books recently published were designed primarily for the use of those counselors at the high school and college level whose formal training has not been extensive. These include Erickson's manual (7), Rosenzweig & Kogan's introductory psychodiagnostics text (8), Rothney & Roens' volume on counseling of individual students (9), and Hoppock's text (10) on group guidance techniques.

Differential and Special Aptitude Tests

The publication of three supplements to the Psychological Corporation's manual (11) for the Differential Aptitude Tests has made available essential

validity data for these tests. Correlations between school grades and the aptitude test scores are presented along with a summary and interpretation of other validation studies.

Prediction of success in engineering schools has been the objective of several important research studies during the past year. Lord, Cowles & Cynamon, (12) using the Pre-engineering Inventory, Revised Form A, have reported significant correlations between the composite score in the Inventory and course grades, between the composite score and grade averages, and between composite score and engineering achievement examinations. Treumann & Sullivan (13) administered the American Council on Education Psychological Examination, Iowa Silent Reading Test, and the Engineering and Physical Science Aptitude Test to a group of engineering freshmen and found the latter to be the best single predictor of freshmen engineering grades; the American Council on Education Psychological Examination proved to be the next best, (although the Q score of this test was low in predictive value). Moore (14) reviewed the attempts which have been made to predict scholastic success in engineering schools over the last 20 years and called attention to the substantial portion of the variance in academic achievement still not accounted for. He attributes the lack of improvement in predictive efficiency of the tests and other instruments which have been developed to the failure to include motivational factors in the regression equation. One of the more promising new tests for use with engineering students is a mechanical comprehension test developed by Owens (15). This test was designed to be more difficult than the Bennett Test of Mechanical Comprehension so that it would discriminate at high levels of ability. The test was administered to a group of 321 engineering freshmen at Iowa State College. The corrected odd-even reliability was .80 and the correlation with the American Council on Education Psychological Examination was .39. The test proved to be an excellent predictor of academic mortality for this group and functioned well as a measure of mechanical comprehension at the upper levels of ability.

Borg (16) attempted to predict art school success, using the Bell Adjustment Inventory, Thurstone Test of Mental Alertness, Test of Mechanical Comprehension (Bennett), and the Strong Vocational Interest Blank for Men as predictive indices. None of the tests was found to be very effective. Significant data were found showing differences between the art students and norm groups. While these data should be valuable for guidance purposes, the tests themselves did not show sufficient correlation with art grades to warrant their continued use as predictors of probable success in art courses.

Measures of Achievement and General Intellectual Aptitude

Since the publication of the Wechsler-Bellevue Intelligence Scale and Rapaport's (17) volumes on diagnosis, there have been many studies conducted concerning the diagnostic value of the Wechsler test. Wittenborn (18) studied Rapaport's data in an attempt to evaluate the use of subtest scores in diagnosis. He concluded that

among Rapaport's diagnostic groups a profile or scatter analysis of the Bellevue-Wechsler subtests is of no greater value as a psychometric supplement to diagnosis than an inspection of the subtests for a few conspicuous instances of failure and relative success.

Gilhooly (19) arrived at a similar conclusion in discussing Rapaport's signs of neuroses. He concluded that accurate diagnosis comes only from "clinical acumen" in evaluating the test performance and the behavior of the individual.

Some doubt has been cast upon the usefulness of the "hold" and "don't hold" tests suggested by Wechsler as measures or indicators of deterioration. Hunt (20) found that the subtests, Information and Comprehension, are the only ones that seem to hold up well with increasing age. He suggests that the above-mentioned "hold" tests should be used in conjunction with the "don't hold" tests, Block Design, and Digit Symbol, in the measurement of deterioration.

Webb & Haner (21) have suggested that the vocabulary subtest of the Wechsler-Bellevue could be used more effectively by rating on a scale of "goodness" the definitions of the words comprising the test. These investigators found that competent judges could obtain more precise data from the subtest with a more refined scoring technique than can be obtained by the use of the present scoring system. An item analysis of the Wechsler-Bellevue by Jastak (22) led to the suggestion that the order of items in several subtests, namely, picture completion, comprehension, similarities, information, and vocabulary, should be changed so that they would be listed in the order of difficulty. Jastak also noted some striking sex differences in item difficulty and suggested that an investigation of the clinical significance of the items showing the greatest sex differences might reveal some important information for diagnosis.

A note concerning the use of the various short forms of the Wechsler scale has been published by McNemar (23). He showed that for 355 cases of Wechsler's standardization population, the "validity" of the abbreviated test is seriously reduced. Cotzin & Gallagher (24) tested mental defectives with various proposed short forms of the Wechsler-Bellevue. They point out the inherent danger of standardizing a scale on one sample of a population and then applying it to another clinical group.

The long-awaited Wechsler Intelligence Scale for Children has recently made its appearance with a rather complete report of the standardization procedures employed and a description of the norms prepared by the Psychological Corporation. Seashore, Wesman & Doppelt (25) have given some additional information relating to the standardization of this test. More results of the analysis of data collected during the last five years have been promised by the Psychological Corporation for early publication. The scale, together with the published reports, should contribute positively to the diagnosis of intellectual aptitude in children.

Wallace (26) conducted a study of the predictive value of the American Council on Education Psychological Examination for University of Michigan Freshmen. Correlation coefficients showing the relationship between grades in various courses and the Q, L, and total score of the American

Council on Education Psychological Examination are reported. Wallace suggests that

> The lack of any marked differences between the predictive values of these two parts in most typical university Freshman courses indicates that considerable caution should be exercised in the interpretation of the Q and L differential for purposes of educational guidance.

Other attempts to predict academic success have been made, using a variety of techniques. Di Vesta, Woodruff & Hertel (27) suggest the development of an inventory with items centered around the areas of motivation, adjustment, and study habits. They believe that prediction of college success can be made more accurate by use of this type of inventory in conjunction with standard achievement or "intelligence" measures. These investigators used an Orientation Inventory to obtain some data on the validity of their suggestion. Altus & Thompson (28) made a study of the relationship between the Rorschach and two different measures of "intelligence." They conclude that

> a large number of M's in an individual Rorschach protocol appears to be prima facie evidence of relatively superior intelligence; the converse does not, however, hold true since some individuals of quite adequate intelligence show few or no M.

Another suggestion for better prediction of school success has been made by Hadley & Kennedy (29). These investigators used a sentence completion test as an indicator of conflict in their subjects. They feel that the use of the sentence completion test in the identification of those students whose poor college work may be attributable to emotional disturbance is promising.

Measures of Interest and Personality Characteristics

The measurement of interests and personality characteristics and the use in counseling of data obtained from these instruments has received increasing attention. Of the interest measures available, the Strong Vocational Interest Blank and the Kuder Preference Record continue to be the most widely used. Several new scoring scales have been developed during the past year for these two interest measures. Kriedt (30), in an extensive study, developed a new scoring key for "psychologist" on the Strong Inventory. He also constructed four new sub-keys differentiating between clinical, experimental, guidance, and industrial psychologists. Baas (31), using a smaller sample of psychologists and graduate students in psychology, reports characteristic profiles in the Kuder Preference Record for these groups. A new scoring scale for women music teachers has been developed by Kleist, Rittenhouse & Farnsworth (32) for the women's form of the Strong Vocational Blank. The authors do not consider their scale to be in final form, however, since a sample of only 75 cases was used in its construction. A discussion of the scales available for the Strong Inventory in measuring the interests of accountants has been reviewed by Strong (33) in a recent article. Strong reports the development of a new "Senior CPA" scale and a title change of the old "CPA" scale to "Partner CPA" scale. Borg conducted two studies relating success of advanced students in the California College of Arts and Crafts to measured interests. In the first study (16), Borg found no rela-

tionship between scores on the Strong artistic scale and grades in art courses. In his second study (34), he reports characteristic profiles for art students on the Kuder Preference Record. Here again he found a low correlation between art grades and art interest scores, but attributes this finding to the homogeneity of the art students with respect to level of art interest.

An interesting, practical application of the Kuder Preference Record was attempted by DiMichael (35). He administered the Kuder to 146 vocational rehabilitation counselors and related the scores to the work satisfaction of the counselors, as indicated by a Survey Sheet, and work efficiency, as rated by the supervisor. The magnitude of the correlations obtained was not large enough for use in individual prediction, but there were some significant relationships between certain aspects of the counseling job; e.g., higher scores on the Kuder Persuasive scale seemed to indicate greater efficiency in promoting the work to the general public, and high Literary scores seemed to indicate greater efficiency in knowing the recent literature on rehabilitation.

The relationship between self-ratings of interests and interest test scores on the Strong and Kuder blanks was studied by Berdie (36). His results are in general agreement with those of previous studies. The scores on the Kuder tend to have a closer relationship to self-ratings than do the scores on the Strong. Berdie concludes as follows:

> In no occupational area, however, was there close enough agreement between measured interests and self-estimated interests to suggest that counseling can be done on the basis of one or the other. As long as measured interests have relevancy for vocational satisfaction and as long as self-estimated interests play an important role in the vocational deliberations of individuals, both types of interests must be considered.

Brown (37), using a group of 50 veterans, studied the reactions of clients to their measured interest scores on the Kuder Preference Record. In general, he found that clients showed a high degree of "confidence" in the Kuder scores. However, no information was obtained as to whether or not the usefulness of interest test scores is increased by obtaining client evaluation of the scores. Brown does report that interrogation of the client presented the counselor with valuable information and increased client participation in the counseling situation.

Several studies of the relationship between interest test scores and school success have been made. Ostrom (38) found no relationship between the Strong Occupational Level score and high school grades, but he did find that the Occupational Level score was related to out-of-school and co-curricular drive as measured by teacher ratings, interviews, and questionnaires. Romney (39) found a low but significant relationship between Kuder Literary scores and college English grades. The addition of the Kuder to the American Council on Education Psychological Examination did not increase the size of the multiple correlation coefficient used in predicting these English grades. Phillips & Osborne (40) also found little relationship between the Kuder scores and scholastic success. However, differential patterns of interests were found among students in the various curricula of the

School of Business Administration and between business and non-business administration students. Shaffer (41), at Indiana University also found significant differences in the Kuder interest patterns of students in the various curricula of the school of business. Another attempt to predict grades in college from interest patterns on the Kuder was conducted by Hake & Ruedisili (42). Using University of Wisconsin freshmen as subjects, some positive and some negative correlations were found between course grades and Kuder Interest scores.

Guilford, Shneidman & Zimmerman (43) have constructed an Interest Survey which yields a profile of 9 categories of interests, measuring 18 special interest traits. The Survey uses a type of item in which the subject gives a direct response to various typical activities or aspects of generic interest fields. A unique feature of the Survey is the dual score for each of the 18 special interest traits, namely, a vocational and a hobby score. A preliminary study of the reliability of the Survey indicates that it may be a useful addition to the interest tests that are now available. Further research upon the instrument is to be reported in the near future.

Another interest measure, the Minnesota Vocational Interest Inventory, to be used in differentiating between the interests of various skilled trades workmen, has been constructed by Clark (44). Scoring keys for this Inventory have been prepared to differentiate members of a trade group from tradesmen-in-general.

Some criticism has been directed at interest inventories such as the Kuder Preference Record, Thurstone Interest Schedule, and the Lee-Thorpe Occupational Interest Inventory, because the choice between occupational title preferences required in these measures ignores the prestige value and monetary return associated with various occupational titles. However, in the construction of the Occupational Interest Inventory, an attempt was made to have the activities in each item matched roughly for job level. Fehrer & Strupp (45), using a population of college students, constructed a list of occupational titles to determine the effect of prestige value in the selection of an occupational title. These investigators, on the basis of their study, conclude as follows:

> Although differences in prestige are recognized, as demonstrated by the fact that the occupations can be scaled for this variable, occupational preferences are not determined by this factor. Instead, preferences are apparently determined by far more basic interest patterns, and, as the consistencies of the scales suggest, these interest patterns constitute a relatively stable component of the personality.

Personality tests such as the Bell Adjustment Inventory, the Bernreuter Personality Inventory, and the Minnesota Multiphasic Personality Inventory have been used extensively in the diagnostic phase of counseling. Arbuckle (46) reported the use of the Bell as a first step or "means of entry" in the counseling process. Arbuckle administered the Bell Inventory to students and invited them to come to the counseling office for test interpretation and found that over half of the students did report. Two-thirds of the students whose Bell patterns indicated the most likely possibility of maladjustment reported for test interpretation. As an aid in the interpretation of

the Bell Inventory, Taylor & Capwell (47) have reported norms for high school students.

A series of studies conducted by Donceel, Alimena & Birch (48) shows the influence of suggestion upon the responses given by subjects to items in the Bernreuter Personality Inventory. A marked copy of the inventory (answers obtained by mere chance) was given to each student participating in the study, under the guise that this was an analysis derived from a graphologist who had examined the student's handwriting. For over half the cases, the agreement between the subject's responses and the "analysis" was beyond that which would be expected on the basis of chance alone. Using as a second type of guise the analysis obtained from the Rorschach and Murray Thematic Apperception Test, all of a group of 50 subjects agreed with the "random" analysis beyond the 5 per cent level of confidence.

The relationship of interest variability as measured by the Kuder Preference Record and adjustment status as measured by the Bell Adjustment Inventory was investigated by Klugman (49) in a population of veterans who had applied for guidance. He found that the spread of vocational interests as measured showed no relationship to general adjustment status. There was, however, a slight tendency for the older, more educated, and more intelligent veterans to have a greater variability of interests than the younger, less educated, and less intelligent veterans. Klugman concludes that there is no basis in this study for inferring a relationship between interest variability and adjustment.

The Minnesota Multiphasic Personality Inventory (Booklet form), the Strong Vocational Interest Blank for Men, the Kuder Preference Record, and the Bell Adjustment Inventory were administered to 400 adult male veterans by Cottle (50). By examining the intercorrelation matrix of the scores on these instruments, Cottle found that there was little significant overlap between interest measures, the Strong and the Kuder, and the personality inventories. He did find relatively high correlations between logically related scales on the interest inventories.

As indicated by Berdie (1), the use of the Minnesota Multiphasic Personality Inventory in counseling has increased markedly in recent years. Clark (51) found that students who scored higher by 20 points or more on the language section of the California Test of Mental Maturity than they did on the non-language section, scored significantly higher on the Hs, Hy, Mf, and Pa scales of the Minnesota Multiphasic Personality Inventory. Daniels & Hunter (52) suggest that the Minnesota Multiphasic Personality Inventory might be used more extensively in vocational guidance. They state that

> These "personality demands" once established (by analysis of various occupations) could then be matched with the "work needs" of the total personality as indicated on the Multiphasic patterns in a manner similar to that already established in the occupational realm of job demands and physical capacities analysis.

In other words, they suggest that occupational ability profiles should be extended to include the personality demands of the occupation as well as the abilities required.

Questionnaires, Check Lists, and Projective Techniques

Kerr & Martin (53) conducted a study for the Indianapolis plant of the RCA Victor Division of Radio Corporation of America to determine the relationship of various items of personal history to job success. They used raw merit ratings by supervisors as the criterion of job success. The entries on the personal data blank were treated as dichotomous scores. The tetrachoric correlations ranged from .30 for married versus non-married workers to −.27 for "weight of males." Although none of the correlations for this group of 224 workers is high, the authors state "it seems reasonable that the application blank or systematic autobiographical inventory should become a standard part of the psychometric battery in industry."

The Mooney Problem Check List, College Form, was administered by Gordon (54) on two occasions to a group of subjects with a nine-day interval between the administrations. A statement of problem changes of these students during the nine-day interval was obtained independently. It was found that on scoring both administrations of the check list that the "problem itself was indicated 97.8 per cent of the time, the direction of change being reflected 82.6 per cent of the time." Gordon concludes that the Mooney Problem Check List is a highly sensitive instrument to identify problem changes as indicated by this independent measure.

The use of a projective technique in the diagnosis of learning disabilities of children was investigated by Monroe (55). She used as the diagnostic instrument a modification of the TAT adapted to the needs of children. Monroe found that a child confronted with the problem of writing about other children with similar difficulties, pictorially described, tended to project parts of his own history. Monroe suggests that this technique is useful, not only as an aid in therapy, but also as a helpful device in the diagnosis of the problem.

Rotter, Rafferty & Schachtitz (56) adapted an incomplete sentence test to appraise the degree of conflict or maladjustment among college students. A separate manual for scoring was developed for males and females. Reliability coefficients were obtained by correlating two independent scorers. In the case of the male subjects, the reliability coefficient was .90 and in the case of the female subjects, .96. Biserial validity coefficients between rated adjustment and test score were .64 for females and .77 for males. The use of this type of projective technique appears to possess some promise for measuring degree of conflict or maladjustment in college students.

The Interview

The interview occupies a basic role in the total counseling process. Unfortunately, research on the interview is difficult and hence is not undertaken by many investigators. Attempts at evaluating the interview have been complicated by the difficulty in establishing a unit for analysis. Robinson (57) has reviewed the techniques which have been used for this purpose such as single speeches, whole interviews, tenths of an interview series, etc. Robinson suggests that the "topics" discussed in an interview may constitute meaningful units. He presents some data justifying the use of the "topic" as a unit in interview analysis.

Davis & Robinson (58) analyzed recordings of counseling interviews in an attempt to discover the counselors' techniques for gaining and maintaining rapport throughout the interview. The study lists some of the techniques most frequently used by the counselors at Ohio State University, but does not attempt to designate the techniques that are most effective.

The validity of work histories given by the applicant was the subject of a study conducted by Keating, Paterson & Stone (59). A total of 236 cases was selected at random from the United States Employment Service, St. Paul office, for a study of the accuracy of the applicants' reports of past employment in relation to weekly wages, duration of employment, and job duties. The investigators found the clients' reports of work history to be very accurate and estimated validity coefficients as being from .90 to .98.

A study to indicate the improvement in academic performance of students who had been interviewed by the instructor was attempted in a class of 257 by Sherriffs (60). A random selection of 34 students was interviewed by the instructor under the guise that he wished to know something of the backgrounds of his students. This interview was conducted between the two mid-term examinations. Sherriffs found that the experimental group scored significantly higher than the remainder of the class on the second mid-term multiple-choice examination (at the 2 per cent level), but that on final examination they did not score significantly higher (at the 5 per cent level). During the interview the instructor rated the students on some personality variables; differences were noted, but the sample proved too small to disclose significant relationships.

Summary

In his recent review of the literature, Berdie (1) listed the following trends as appearing in recent publications pertaining to diagnosis in counseling:

(a) Individual workers are publishing comprehensive and frequently critical surveys and reviews of diagnostic tests; (b) attention given during the war to the development of brief, screening tests has continued; (c) continuing work is being done on the development of tests of special abilities; (d) professional aptitude tests are being developed in new fields; (e) implications for the interpretation of personality found in interest tests are being explored; (f) attempts are being made to develop new uses in counseling for the Minnesota Multiphasic Personality Inventory; (g) new uses of projective methods in counseling are being explored; and (h) lacunae in the research literature continue to reveal the lack of evidence concerning the validity of many otherwise carefully developed diagnostic tests.

The trends observed by Berdie continued to appear to some degree in the literature of the past year. Perhaps the most notable are the following: (*a*) continued interest in the use and validation of differential aptitude tests, (*b*) search for better methods of assessing personal qualities, particularly interests and personality characteristics, and (*c*) better standardization and validation of existing tests and diagnostic devices.

The amount of progress in the development and use of diagnostic pro-

cedures which occurred during the past year is in some ways disappointing. No radically new techniques were developed and no particular progress seems to have been made in relating diagnosis to therapy or remedial procedures. The progress which has been made is primarily in connection with the better interpretation and use of earlier developed instruments. This is, of course, an important type of progress. By learning how to use an instrument more effectively or by extending the range of usefulness of instruments developed, diagnosis in counseling becomes more significant. In a sense, any study of the construction, standardization, or validity of an instrument makes a contribution to diagnosis in counseling. The total progress in the field may, therefore, be more extensive than would appear to be the case from a reading of this chapter. One can be sure, also, that studies are now under way which will result in new and better techniques and in more effective use of techniques and instruments already in existence. There is in other words, reason to believe that diagnosis in counseling will become increasingly accurate, its role in the total counseling process clarified, and its relationship to therapy and remedial action made more precise and useful.

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COUNSELING METHODS: THERAPY[1]

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In the period covered by this review, contributions to knowledge of therapy in counseling fall within several areas: (*a*) attempt at definition of counseling as a profession, (*b*) formulation of theory and method, (*c*) analysis of the counseling process, (*d*) discussion of therapy in groups, and (*e*) research and summary of research on the effects of counseling.

Counseling as a Profession

Four recent texts emphasize the uncrystallized state of the field. Hahn & MacLean (1) have described the general clinical counselor as a "general practitioner" in human problems. According to these authors, general clinical counseling ". . . is a process which takes place between an individual troubled by personal problems with which he has been unable to cope alone, and a professional personnel worker whose training and experience have qualified him to aid others to reach solutions to various types of personal difficulties" (1, pp. 3–4). To these authors, also, general clinical counseling is that practice of clinical psychology which is found primarily in schools, colleges, and universities; in a nonmedical setting, the general clinical counselor diagnoses and treats a wide variety of human problems within the "normal range" of adjustment. The recommended training programs draw in large part upon areas of preparation prescribed by the American Board of Examiners in Professional Psychology (2) for certification of "counseling and guidance psychologists." The importance to the trainee of carefully supervised observation and internship is stressed.

Hahn & MacLean (1), along with Super (3) and Williamson (4), imply that a strong background in differential psychology is essential to effective counseling. They say that the kind of help one gives a client should depend, at least partially, upon scientific methods of appraisal, e.g., of his abilities, interests, and attitudes, by means of which his performance can be measured and compared with that of appropriate norm groups.

In Porter's *Introduction to Therapeutic Counseling* (5), the training of the "therapeutic" counselor is centered on a study of: (*a*) the attitudes and perceptions of the client—most importantly how he feels about himself as revealed in his actual statements during counseling interviews, and (*b*) the attitudes and perceptions of the counselor in responding to what the client says. Porter has set forth with clarity a system of describing and categorizing counselor responses. His categories have seemed to follow empirically from the study of verbatim records of interviews. One may criticize Porter as having prescribed a counselor training program which is too narrowly cen-

[1] This review covers the period from June, 1949 to May, 1950.

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tered upon client-counselor verbalizations in interviews, since he gives no evidence that other data are either relevant or necessary.

Helpful contributions to counselor training have been made by each of the four texts mentioned in this section. Hahn & MacLean (1) have presented a description of general clinical counseling which warrants wide acceptance among professional psychologists as a broadly useful and definitive exposition about what a counselor is, should know, and does in his contacts with clients. Super (3), while restricting his discussion to vocational appraisal, presents an excellent discussion of tests and their uses in counseling with clients about vocational plans. Williamson (4) effectively describes the historical background of counseling, and discusses in broad perspective the function of counseling in the college or university system. Porter's (5) manual for interviewers is exceptionally clear and well-organized; his ingenious pre- and posttraining tests are valuable training aids.

Baier (6) has suggested an "integrated, cooperative research attack" on selecting, training, and evaluating performances of counselors and other guidance workers. Wrenn (7) and Darley (8) have provided eight criteria "for appraising the extent to which student personnel work is moving toward professional status." They conclude that no part of the program has achieved mature professional status, although some phases of the personnel program, e.g., counseling, have become more professionalized than others.

Darley (8, 9) warns that professionalization is a mixed blessing. For example, he cites the rigidity, immobility, and extreme conservatism of those who "have at some cost to themselves over the years captured a vested interest," and states that the profession of counseling affords illustrative cases. Darley's detachment is refreshing. He might have added a note about increasing complexity, difficulty, and length in time of the "rites of passage" which lie ahead of the would-be counselor who seeks to move from novitiate to professional status. A case in point is the comprehensive manual on "counselor preparation" (10) which was prepared by representatives of eight professional groups under the sponsorship of the National Vocational Guidance Association.

A valuable book of readings edited by Brayfield (11) reflects the broad scope of articles on counseling published during the last decade. Reporting on books and articles published in 1948 to 1949, Bordin (12) noted a broader concept of counseling than that of helping the client to make educational and vocational decisions. Books and articles published in 1949 to 1950 indicate increasing concern with social and psychological factors relevant to the counseling process. Whether counseling will be integrated "as a special subdivision of psychotherapy" as argued by Bordin (11) and others, may be questioned by those who hold that "psychotherapy" is too restrictive to describe what the counselor does with clients. Hahn & MacLean's idea (1) of the clinical counselor, as a clinical psychologist who is primarily employed in educational institutions and who functions as a "general practitioner," may find more widespread acceptance.

Theory and Method

The most significant development in publications on counseling during the past year has been a concerted attempt to integrate what happens in therapy with basic theory and experiment in the social sciences, especially in general psychology. Reference is made to motivation, learning, and perception, and also to the seemingly more diffuse areas of personality and social organization. An interesting application has been made of concepts drawn from the study of general semantics. Later scrutiny may show these attempts at integration to have been primitive and inaccurate. In the meantime, the careful and extended reading of source materials in basic fields is strongly recommended. For example, the counselor who applies learning theory to account for what happens in therapy should be aware of disagreements between "reinforcement" and "nonreinforcement" theorists, where cogent argument backed up by rigorous experiment is advanced in support of opposing points of view.

Perception.—The apparent selectivity with which the human organism perceives itself and its surroundings (13) has been emphasized by one group of psychologists as the basic datum of therapy with clients. Snygg & Combs (14) have described a "new frame of reference" in therapy: a concern with how the client perceives and reperceives his self in its relations to its environment. Porter (5) supports this "internal frame of reference" and hypothesizes that "each person has within himself the capacities to order and reorder his own behavior without external motivation by another." The function of therapy is to allow the client to explore and alter his own motivations through the accepting and understanding reflection of his feelings by the counselor.

In Porter's (5) book, the "internal" frame of reference is applied with compelling simplicity and coherence to the study of interviews and of the therapeutic process. Whereas Porter speaks of the central nondirective "hypothesis," implying a need for further experiment, Snygg & Combs (14) and Combs (15) argue on the level of "theory," with the implication that a large, systematically ordered body of experimental evidence exists in support of their point of view. This does not seem to be the case.

Ichheiser's (16) monograph is a pertinent caution against the uncritical application of the "new frame of reference" in therapy. He maintains that many misunderstandings in human relations arise through false social perception. The "expressions" of one person are received as "impressions" by another. Errors in observation may arise because (*a*) it is difficult to know what a person really means or manifests of himself by his "expressions," and (*b*) "impressions" of these by another person are themselves subject to distortion through selective perception of the observer.

One wonders to what extent the "internal frame of reference" approach to therapy is a rededication to the subjective idealism of prescientific psychology. In this system, we conceptualize for each person a separate world of

space and time which exists only because he perceives it, and in which he lives *solus ipse*. Yet we may look upon this development as a wholesome reaction away from the "objective appraisal" and information-giving idea of counseling of the 1920's and 1930's.

Like Snygg & Combs (14) and Porter (5), Hilgard (17) proposes that human motives be studied in terms of the self-concept. Unlike these writers, however, Hilgard urges that we go beyond the study of self which is present in awareness (the perceived self) to an inferred self. It is hypothesized that the job of the therapist is to help the client achieve insight (*a*) into the nature of self-distorting mechanisms or (*b*) the conditions which have made them necessary. This hypothesis, according to Hilgard, implies that the client is helped to re-evaluate his self and its motives, to accept previously unacceptable features of the self. The distorting, previously protecting mechanisms will "simply dissolve" if the individual can now have more security without than with them. These conceptualizations of the self and its role in human motivation provide the possibility of a broader, more accurate understanding of human behavior than we now have as experimentalists or as counselors. The therapist may be able ultimately to use knowledge of the self in predicting and controlling the behavior of the client. Prediction and control are basic to any therapeutic rationale which purports to describe how clients are helped to make more satisfying adjustments, as well as how they are not helped. Even the nondirective counselor, in Hogan's (18) terms, does everything he can to direct the client's attention to himself, and, as Porter (5) and Combs (15) have implied, to free the client to explore his own attitudes and feelings. The nondirectivist further stipulates the antecedent conditions which will—predictably—allow the client to redirect his activities in ways that are personally satisfying to him.

A novel application of the concept of perception involving deliberate use of the Rorschach as a method of therapy, purportedly comparable to psychoanalysis or nondirective counseling, has been proposed by Rashkis (19). His idea is that changing responses on the Rorschach will change the perceptual environment of the client. Rashkis assumes that since all psychological behavior is based on perception, then changes in Rorschach responses, whether mediated by the client or the therapist, will change those aspects of the client's behavior with which the Rorschach responses are correlated. In this therapeutic method, called "perceptsynthesis," two personality pictures of the client are obtained: (*a*) by administration in the usual manner ("present mode of perception"), and (*b*) by testing the limits ("potential mode of perception"). In succeeding administrations of the Rorschach, the therapist "reinforces" responses considered "desirable" and "negatively reinforces" those considered "undesirable." Just what Rorschach responses are correlated with what behavioral changes in the client, and how permanent the latter might be, are considered by Rashkis to be problems for further research.

Rashkis concludes that "perceptsynthesis is a straightforward psychological technique with a theory based on scientific psychological principles." Some psychologists might infer that perceptsynthesis is a method of therapy

akin to wagging the dog's tail to make him happy. A fair trial by experiment is strongly recommended.

Learning.—Learning is the area of general psychology that appears to hold the greatest promise for a rational, communicable conception of what happens to the client in counseling. Moreover, the extension of learning theory to the counseling process has helped to create a bond between clinician and experimentalist. In this relatively new partnership, ideas can be exchanged and hypotheses tested, with results potentially useful in theory construction and clinical practice alike. With due allowance for bias within the reviewer, it is significant that there appears to be the most cautious pronouncement, the least sweeping generalization, among writers who are concerned with therapy as a kind of social learning. Seeman (20) and Shaw (21), writing in the same issue of the *Journal of Abnormal and Social Psychology*, have provided valuable notes in this connection.

During the past year, important articles on therapy as learning have been published by Shaw (21, 22), Shoben (23), and Magaret (24). Shaw (21) points out that a variable such as "anxiety," which cannot be directly observed, can be a legitimate part of an empirically derived, logical system for understanding and predicting behavior. In such a system, the variable "anxiety" can be inferred and predicted within the human organism from a set of known, antecedent, observable responses of the organism. The value of an inferred "anxiety," which operates as an "intervening variable," is that it can be experimentally manipulated through systematic variation of the antecedent conditions. The relationship between different amounts of "anxiety" (independent variable) and responses of the organism (dependent variable) can thus be studied. Shaw's stated purpose in this paper is to illustrate how theory can serve the clinician.

Both Shaw and Shoben (23) discuss therapy in terms of Mowrer's (25) two-factor theory of learning. Shoben's remarks are addressed to counselors working with clients whose syndromes are essentially neurotic in character, involving feelings of insecurity, inadequacy, and guilt. To Shoben, these feelings are manifestations of anxiety. Since anxiety has been shown to have drive properties, it may be inferred that nonintegrative behavior results from the client's attempts to reduce the underlying anxiety. The counselor must work with the client to help him modify his anxiety. This is done in three ways: (*a*) by allowing the client to reinstate symbolically the anxiety-provoking stimuli; (*b*) by allowing the client to reduce anxiety through attaching the anxiety-producing stimuli to the response of comfort in the interview (counter-conditioning); and (*c*) by working with the client to help him formulate rational goals and ways of behaving to reach them (symbolic trial-and-error developing, through reinforcement, into integrative behavior). In Shoben's own terms, his formulation "leans rather too much on plausible but inadequately tested hypotheses and on scientifically tenuous analogies." Nevertheless his article is an example of the kind of thinking counselors ought to be doing as a guide to research and to practice in therapy.

An illustration of how Shoben's "counter-conditioning and reinforcement"

hypothesis might be applied in the counseling interview is provided by Butler (26), who proposes a counseling approach in which both "non-directive and directive techniques may have maximum utility." Butler's notion of the "adjustive" phase of counseling, in which the client is helped to gain in self-understanding and acceptance, may be thought of in terms of Shoben's "counter-conditioning" stage of learning in therapy. Butler's "distributive" or "teacher-student" phase, in which the client can be given access to relevant information, may be likened to Shoben's "reinforcement" stage of learning.

Magaret (24) presents another interesting and valuable contribution to therapy from learning theory. Her thesis is that the client generalizes at an increasingly higher level of integration from the learning of interpersonal reactions with the therapist to interpersonal problems outside the therapeutic relationship. In successful therapy, a "learning set" is achieved by the client in which he learns "how to learn" successful interpersonal reactions.

The notion that "insight" may emerge as symptomatic of changes that have already occurred in therapy has been proposed by Hobbs (27). He maintains that therapy which helps the client to explore important life relationships through the use of symbols which he perceives as pertinent are most likely to promote change. The weakness of this kind of argument, as Shoben (23) has indicated, is that it seemingly helps to explain only how the client can reinstate, symbolically, anxiety-provoking stimuli which have been repressed. A similar weakness appears in Hilgard's (17) notion that self-defending, nonintegrative mechanisms might be expected to disappear as the "inferred" self was understood and accepted by the client. In counseling we should be concerned with how actual behavior changes take place as the result of therapy, especially as these behavior changes are promoted outside of the counselor-client relationship in the interview. The hypotheses of Shoben and Magaret appear to be more adequate than those of Hilgard and Hobbs to explain how such changes can take place in successful therapy.

Social factors.—The importance to the counselor of background in cultural anthropology and social psychology is given emphasis by Williamson (4), who states that this may give the counselor a useful framework of concepts and hypotheses. Williamson conceives the goal of counseling to be that of helping the client to obtain satisfaction in life from maximum achievement within the limits of his own capabilities and the restrictions imposed on him by society. If this follows, then the counselor should know a great deal about the client and about society. Cultural determinants as limiting factors in counseling have also been stressed by Darley (9).

Illustrating a way in which social determinants of behavior may be incorporated into theory construction relevant to the counseling process, Levin (28) hypothesizes "status anxiety" as one of the primary motivational factors bearing on the occupational choice dilemma of many clients. He cites this and other "basic needs" of the client as implying—at least in some cases—the desirability of a depth approach to vocational counseling. Levin cautions against the inference of a universal "status need," even in our society.

Social class determinants may be relevant here because "having restricted achievement needs and low motivation educationally and vocationally may have quite different diagnostic (*ergo*, treatment) implications in different social classes." That the very pattern of participation in counseling may need to be varied with the social class membership of the client is suggested in an intriguing discussion of "participation, culture, and personality" (29). A search for hypotheses and research needs to be conducted in this area—implying a partnership of social psychologist and counselor.

General semantics.—An innovation in counseling technique, the use of general semantics in diagnosis and therapy, has been introduced. For example, Hahn & MacLean say (1) "It seems clear that many clues to counselees' difficulties and many means of finding ways out of them lie in their written and spoken language . . . ," and Williamson (4) remarks that "another methodology has been added. . . . Experience and experiment will tell us which method is effective, with what problems, and under what circumstances and conditions." There appears to be in therapy a connection between problems in the study of language and those in perception.

Methodology.—A critical review by Ellis (30) centers on the coordinated research of the Chicago nondirective counseling group (31), citing this as "an excellent start towards scientific research standards." On methodological grounds, however, Ellis criticizes this research on the following points: (*a*) "naively accepting patients' statements at their face value," (*b*) use of "vague and meaningless" concepts for understanding therapeutic cases, (*c*) incomplete descriptive or diagnostic data concerning case studies, (*d*) *ex post facto* design using data collected prior to the formulation of the research project, (*e*) analysis of protocols rather than clients, (*f*) inadequate number and representativeness of cases sampled, and (*g*) omission of relevant "concomitant qualitative and sequential analyses of sessions of psychotherapy." While his review is constructive and helpful in the main, it is less effective in telling what to do than in telling what not to do in research on psychotherapy. Shoben (23) has aptly described limitations imposed upon research design in the counseling center, since the counselor is still under obligation to give service to his clients. Moreover, current budgetary limitations on available personnel and facilities are real barriers to a comprehensive, coordinated research program. One wonders for example, whether Ellis is truly aware of the enormous labor and financial expense involved in collecting complete recordings of interview sessions or in analyzing them. In his final criticism Ellis himself becomes vague. For example, an experimentalist would be hard put to attempt "the contextual analysis of the patient's statements and behavior, and their interrelationship to the gestalts of his entire therapeutic and life processes." These, in Ellis' own terms, are "admittedly difficult problems to undertake." Bordin's (12) appraisal of Travers' (32) attack on evaluations of "guidance" services would seem to apply here: " . . . this argument apparently oversimplifies the problem by underestimating the tangled jungle of issues of general and personality theory, as well (as) of counseling methodology."

From the standpoint of experimental method, one of the neatest studies yet to appear in the literature on counseling is that of Keet (33). Using as a dependent variable a learning task based on Jung's word-association test, Keet was able to devise a miniature counseling situation to check on a subject's ability to recall after therapy a word having reference to an area of disturbance in the subject. The miniature counseling situation was manipulated as an independent variable in two ways: (*a*) the therapist used an "expressive" technique to encourage release of feeling by the subject, and (*b*) he used a combined "expressive-interpretive" technique to encourage release of feeling and to assist the client in recalling the specific affective experience associated with the area of disturbance. Although the experimenter is cautious in drawing conclusions from his results, this study should serve as a prototype of "single factor" investigations of the effects of counseling. Keet's study meets an additional methodological requirement in that his results contribute to hypotheses for further research on regressive behavior. He is able to demonstrate that his results tie in with a conceptualization of regressive behavior in adults; i.e., that regressive behavior results from a breakdown of symbolic processes which otherwise operate as an integrative "functional barrier" between receptor and effector.

Analysis of the Counseling Process

Few significant new trends in analyzing the counseling process have appeared since Bordin's (12) review. Discussion of the directive-nondirective controversy continues, counselor and counselee attitudes are described, and an attempt is made to analyze counselor and counselee behavior in the interview.

Counselor and client roles.—Despite Bordin's (34) earlier suggestion that a multi-dimensional analysis of the counselor's role might be more rewarding than analysis in terms of a single directive-nondirective continuum, several writers still give the impression that counselors vary only in this one respect. Comment has already been made about the position of the nondirectivists, as exemplified by Porter (5), Snygg & Combs (14), Combs (15), and Hogan (18), that the counselor should convey to the client an attitude which is permissive, accepting, and understanding. According to the nondirectivists, this attitude results in a counseling procedure which is helpful yet nonmanipulative, permitting the client to "grow." Anything else is considered restrictive and harmful to the client. A modification of the "either . . . or" position is proposed by Stone (35), who has attempted "a logical analysis of the directive, nondirective continuum." The aid of "advanced psychology students, administrators, and counselors in classes" was enlisted over a three year period in "a program of logical (sic) research." One may question the logic and the validity of Stone's continuum on two grounds. First, he began his research with what appears to have been the postulate—not the hypothesis—that there was a single, definable continuum of counseling method, and second, he has apparently failed to guard against contaminating the thinking of his students by his own preconception. Hahn & MacLean

(1), Williamson (4, 36), Thorne (37), and Bordin (38) state that the counselor must learn to modify his interview procedures as needed in helping the client. The rationale of this group as presented by Bordin (38) states that the counselor should be provided through experiment with "objectively established principles, with concretely described operations, and with predictable results."

Counselor attitude has received frequent mention as an important variable in therapy. Porter (5) has provided the most systematic and exhaustive treatment of this topic without, however, providing evidence for the reliability and validity of his attitude categories. An attempt by McClelland & Sinaiko (39) to construct a paper-and-pencil test based on the Chase Questionnaire (40) proved abortive largely because of the low reliability of the scale. It appears plausible that the method of Porter and his colleagues in using specific illustrative materials from counseling interviews will be a more accurate basis for judging attitude than that of the original or modified Chase Questionnaire which asks for a counselor judgment of the techniques he would consider appropriate in a "general" counseling situation. There is need for investigating the relationship between how a counselor says he would respond and his actual responses in therapy.

A question related to counselor attitude is the role of counselor adjustment in effective therapy. An opinion survey by Seeman (41) reports that "expert" therapists disagreed on whether the "best adjusted" therapist is apt to be most successful in helping his clients, although a majority of 70 judges sampled agreed that the therapist should be given an opportunity to receive skilled help in solving his own personal problems. Psychoanalytically-oriented therapists were shown to be more definite than those nonanalytically-oriented in their opinions as to what is desirable for the therapist. Although Seeman's study has limited value because it was not intended to answer the crucial question of how the counselor's adjustment is related to what he does in therapy, it does emphasize an important area in which research is needed.

Little evidence is available on the attitudes or expectations with which clients approach the counseling process. An article by Arbuckle (42) suggests a possible means of investigating client "set" through analysis of group entrance test scores, but his results are ambiguous. Grant & Grant (43) hypothesize that a client's "therapy readiness" can be used as a variable in therapy research. Two raters analyzed verbatim reports of the initial counseling interviews of six clients, one judge using a crude "readiness rating" scale, and the other simply ranking the cases in terms of "therapy readiness." A rank order correlation of .92 was obtained between the two sets of ratings, suggesting that the "attitudinal set" of clients might be reliably observed.

Analysis of the interview.—The counseling interview can be analyzed to serve varied ends. The principal method of analysis is that of (*a*) obtaining verbatim records of typescripts, and (*b*) determining whether independent observers can agree in analyzing counselor or client statements according to a priori, response categories. The Grant & Grant (43) study is one exam-

ple. Robinson (44) reports that the "unit" of analysis can be determined in several ways, such as: single speeches, whole interviews, tenths of an interview series, and topic of discussion. Each method is said to have its advantages and disadvantages. The most exhaustive, yet highly reliable, method of analysis is described in detail by Bales (45). He has shown how observers can be trained to use his method of "interaction process analysis"—designed for observing participation in groups—in the study of counselor and client responses in a series of counseling interviews. His method, however, suffers from two possible weaknesses: (*a*) it is atomistic in assuming that identifiable response units are additive, yet purports to provide a molar picture of "interaction process," and (*b*) it is too cumbersome and costly to be applicable in most research designs.

Kauffman & Raimy (46) have published the results of a study previously cited by Snyder (47). They report that analysis of clients' self-references in interviews by means of the "PNAvQ Quotient" produces results essentially similar to that of analysis through use of the "Discomfort-Relief Quotient," and in about one-third of the time necessitated by the latter method. Davis & Robinson (48) report a study of "resistance" shown by 22 counselees in 68 recorded interviews conducted by 22 counselors. "Topic of conversation" was used as the unit of analysis. Significantly high rank order correlations were obtained in classification-reclassification and inter-judge comparisons made to determine the reliability with which "resistance-reducing" techniques could be identified. No consistency in type of technique or time of application was found among the counselors, and no conclusions could be drawn concerning effectiveness of technique.

Kamm & Wrenn (49) investigated the conditions under which clients accept or do not accept information about themselves in the counseling interview. Forty clients were interviewed by a single trained counselor in educational-vocational planning sessions. Clients were given pre- and post-interview questionnaires asking them about their plans; then they were reinterviewed by the same counselor one month and four months after the initial recorded interview. Three judges were asked to review all pre- and postinterview data and to decide whether acceptance had or had not occurred in each case. Counselor and client responses in the recorded interviews were categorized. The authors present a major conclusion that client acceptance is related to client feeling, particularly toward himself. They also report, among other findings, that acceptance is related to "readiness to act with regard to a felt need." Unfortunately, for the sake of interpreting or duplicating this study, no quantitative results are presented.

Keet's experiment (33), previously cited, revealed that three judges could agree markedly in differentiating between "expressive" and "expressive-interpretive" interviews conducted by a single counselor. Substantial, but not as marked agreement, was found in the categorizing of responses within each type of interview. Abt (50) reports on the use of a "Guided Clinical Interview . . . to collect personality data about the subject not available from other sources." Subjects were 48 male above-the-knee am-

putees, veterans of World War II. Three clinicians rated client responses in each of five categories of attitude covered by the interview. Although the technique is primarily diagnostic, the discussion of results is relevant to a consideration of accuracy in analyzing interviews. It appears that results as presented in a summary table are far more ambiguous than the author of this report would have us believe in his conclusion that the rating blank "clearly" differentiated among the subjects. There are often as many positive as negative ratings for a subject in a given category, and the N's in the cells are too small to permit valid statistical inference from them.

In summarizing these studies of the interview, several points may be made. First, it would seem profitable to make a further search for relevant variables which can be derived empirically from interviews. Second, publications should include more information about processing and statistical analysis of data. For example, we should know whether many of the research workers in this area who make frequent use of the Chi-square technique are guided by a knowledge of its limitations in application—as aptly clarified by Lewis & Burke (51). Third, counselors should work toward the building of a systematized body of knowledge. Even in so-called coordinated research programs there is little cohesiveness from project to project. Fourth, we need to know more about what happens in interviews when identifiable variables are systematically manipulated.

Special applications of the counseling interview.—Abt (50) and Fishman, Kransdorf & Lifton (52) have reported on interviews with male amputees. Brown (53) discusses the application of general semantics techniques with physical disability cases. Williamson & Foley (54) suggest procedure in counseling with individuals whose behavior has caused them to be referred for disciplinary action. It is argued by Williams (55) that there is no need for special information or techniques in couseling with Negroes since the same "psychological principles involved in personality development and adjustment" are to be found in Negroes and whites. Williams' statements should be regarded as interesting hypotheses.

Several articles center on vocational counseling procedure. Dobson (56) differentiates between clients whose essential problems involve a lack of information or less accessible, emotional disturbances. This writer is somewhat naive in his emphatic assertion "that skillful, nondirective interviewing is the *modus operandi*" while recommending the use of "support, manipulative and protective techniques, and encouragement." The importance of accepting the client as he is instead of trying to make him over in the vocational counseling situation is stressed by Brayfield (57). Ginzburg *et al.* (58) have attempted to provide research evidence against the "overselling" of vocational guidance technique. The inadequacy of their research methods is, however, effectively criticized by Meltzer (59). Gales' (60) discussion of counseling with students who have academic difficulties is pertinent in pointing out that such difficulties may be the result of emotional disturbance, although his recommended counseling procedure might appear superficial.

More traditional approaches to vocational counseling involving voca-

tional diagnosis and use of occupational information have been proposed by Christensen (61), Paterson (62), Shartle (63), and Hoppock (64). While accepting the value of this kind of information and technique, Bordin (38) argues that the distinction between "vocational" and "personal" counseling is artificial.

Therapy in Groups

Though the counselor's primary function is still conceived to be that of working with individual clients, increasing attention is being given to his role as a group therapist. Hahn & MacLean (1), while accepting the counselor's contribution to group activities, warn against the uncritical extension of individual counseling procedure to the group situation. As evidence against vocational counseling in groups, apart from the supplemental use of individual counseling interviews, they cite repeatedly the important study by Stone (65). It is regrettable that so much weight must rest upon this one study, since the group "vocational counseling" was administered in lecture fashion to large groups of students, and since the "appropriateness" of vocational choice was determined solely from data gathered while the subjects were still in school and not from actual success on the job. Nevertheless, Stone's experiment provides a caution against extreme claims for the efficacy of group methods in therapy.

Symptomatic of increase in and application of group methods by counselors was a series of papers on "group techniques in the student personnel program," presented at the 1949 meetings of the Council of Guidance and Personnel Associations. Most of these were published during the past year. The introductory discussion by Super (66) provides an overview of the use of group methods in (*a*) orientation, and (*b*) therapy. Super points out that we are still exploring the possibilities of group methods and that awareness of their limitations will follow. He does not think that group methods can replace individual counseling as the important, central medium of therapy. Pepinsky (67) presents "an experimental approach to group therapy." His thesis is that a program of research in group therapy is a legitimate function of a professional counseling center. The use of group methods in the Veterans' Administration advisement and guidance program is described by Cooperman (68), who points out, however, that vocational objectives are selected in individual interviews. Van Dusen (69) cites the use of group discussion, along with personal interviews, in an adult counseling center. Hoppock's (70) paper deals with a group guidance training program for counselors, and Blocksma (71) discusses the concept of "leader flexibility." All of these papers are provocative, but give emphasis to Super's point that we are still in the exploratory phase of using therapeutic group methods.

Among the few available research papers is that of Bills (72) who studied the progress of eight retarded readers who had participated in nondirective play therapy sessions through successive administrations of reading tests (*a*) before and after a "control" period of six weeks (no therapy), (*b*) after an "experimental" period of six weeks (therapy) which followed the "con-

trol" period, and (*c*) again after a third period of six weeks. Significant gains were found after therapy and persisted six weeks later. But inspection of his tables reveals that the largest gains 12 weeks after therapy were made by four children who had Binet IQ's of 147 or above. In the absence of a control group, we cannot determine how much effect there was upon these highly intelligent children of simply repeating the test. Moreover, gains are reported for other "nonretarded" readers in the class who had not participated in therapy, though no comparison tables are given. This suggests the operation of a "hospital effect" independent of therapy itself.

Pepinsky (73) reports an attempt to study a group of six college students given brief group therapy over a period of five weeks. These students were part of a larger leadership training group of 30 students who met informally for 1 hr. a week throughout the school year. Sociometric tests were given in the larger group during the first, third, and fifth weeks of the therapy period. Proportionately larger gains in choices received (as measured by "role" and "status" questions) were found for the therapy group, although Pepinsky's results must be considered inconclusive because of the small number of cases sampled.

Effects of Counseling

Lack of agreement by counselors on desirable counseling outcomes is understandably related to differences in techniques for assessing changes in the client as the result of therapy. Porter (74) has sharpened the issue by distinguishing between counseling which seeks to help the client obtain appropriate information about himself, and that which attempts to help him achieve a more "integrative" self-concept. Bordin's (38) broader conception of counseling, shared by an increasing number of counselors, would include something of both points of view. Evaluations of counseling effects are limited because of (*a*) many uncontrollable stimuli which impinge upon the client outside the counseling situation, and which may or may not be responsible for changes attributed to counseling, (*b*) difficulty in establishing controls for the many possible factors in the counseling situation which may be related to changes in client behavior, (*c*) lack of adequately tested, systematized knowledge requisite to setting up meaningful criteria of change, and (*d*) lack of adequate criterion measures. In spite of these limitations, a commendable effort is being made to gather objective evidence of change.

The method of follow-up interviews, along with pre- and postcounseling interview questionnaires, was employed by Kamm & Wrenn (49) in the study cited earlier. In Froehlich's (75) study, 279 subjects, representative in age and sex of former clients of a state consultation agency, were interviewed by two carefully selected and trained observers. Relationships significant at the 5 per cent level were found between "occupational" and "personal" adjustment criteria. Significant inter-item correlations were obtained within the "occupational," "personal," and "client attitude" criteria. "Change in status" criteria were independent of each other and of the other criteria. Since these criteria were based on the two interviewers' use of an

interview check list, it is quite possible that "halo effect" and "response set" in answering adjacent items may account for the relationships found in this study. A further limitation in generalizing from these results is the use of client statements as evidence of validity.

Anderson (76) made use of follow-up questionnaires in analyzing responses of 685 counseled employees in an industrial plant. Respondents were differentiated in terms of their ages, education, ability levels, and sex. A majority of respondents of all groups reported favorable effects; although younger, less educated men and older, less educated women tended to report less favorable outcomes. In a second study, 444 veterans who had been interviewed by the counselors of the first project were actually followed up in their job placements. Four-fifths were observed to be satisfactorily placed, and only 3 per cent had changed jobs in 19 months. Possible weaknesses in her report are: (*a*) a failure to specify the representativeness of those who replied to the questionnaire in the first project, and (*b*) the absence of control groups in both projects.

Cowen & Combs (77) followed up former clients of a university counseling service who had voluntarily applied for counseling in 1944 and 1945. Twenty-seven of the former clients had taken the Bernreuter Personality Inventory before counseling and again "after a period of time had elapsed following the final therapeutic interview." Significant mean differences, favoring the post-test, were found on four of the six scales. Inspection of the table from which these results are obtained reveals that pre- and post-test variances are homogeneous for all but one of the scales and that the mean scores for each of the other scales fall within one S. D. of the standardization group mean. One may question, therefore, whether there has been actual "improvement" in test scores. Twenty former clients were interviewed beginning in 1947. In 60 per cent of the cases accurate prognoses could be made from evaluation of counseling records to evaluation of follow-up interviews as to whether "success," "progress," or "failure" in therapy would be shown. This study is commendable because it includes an extended discussion of its own limitations.

Measures of changes in experimental and control groups as the result of individual counseling were obtained by Keet (33), Sherriffs (78), and Dressel (79). In Keet's study (33), described earlier, significant differences were found between groups counseled by "expressive" and "expressive-interpretive" techniques in ability to recall a stimulus word associated with an area of disturbance in the client, favoring the latter technique.

Sherriffs (78) held one-hour "personal" interviews with 34 students "representative" of a class of 257 students in age, sex, and academic major. Although the experimental group did not differ from others in the class in scores on a course examination given before their interviews, they did make significant gains on an examination given four weeks after the interviews. Differences between the groups are small, however, and there seems to be considerable overlap in scores. On the final examination, given 10 weeks after their interviews, the groups were not significantly different.

Dressel (79) describes an experiment in which the results of "a uniform battery of aptitude, interest, and personality tests" were interpreted to "selected" clients in single-session counseling interviews. A test of self-understanding was administered to all subjects prior to their interviews. Immediately after the experiment, the self-understanding test was again administered, along with a questionnaire to determine how the client felt about his progress. The test and questionnaire were readministered three months later. Type and degree of client participation in the interviews was evaluated by four judges who had access to wire-recordings of the sessions. Dressel's brief report leaves some doubt as to the actual experimental design. It is hoped that details and results of the study will be published.

Analysis of changes in client and counselor during counseling interviews is a third method for observing the effects of counseling. This is illustrated in the publications of Bales (45) and Kauffman & Raimy (46) cited earlier. Although these methods have grown out of systematized bodies of knowledge in the social sciences and can provide reliable evidences of changes during counseling, they seem to be abortive unless the results which they yield can be related to other, independent observations of behavior.

Finally, comprehensive, critical reviews of methods for evaluating counseling outcomes may be found in Hahn & MacLean (1) and Williamson (4, 80).

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STATISTICAL THEORY AND RESEARCH DESIGN[1]

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The present review will place primary emphasis upon problems of research design in psychological research. Statistical techniques and methods will be stressed with reference to their potential value in the analysis of experimental data. The reader will, in most cases, have to refer to original sources for details, but a few papers have been selected for a more complete treatment. No pretense is made that the present review covers all papers dealing with statistical theory and research design published during the past year.

Research Designs

Study designs.—Stouffer (1) has presented a general discussion of "study designs" used in sociological and social-psychological research. His ideal model is the controlled experiment which takes the form

	Before	After	After − Before
Experimental group	x_1	x_2	$d = x_2 - x_1$
Control group	x_1'	x_2'	$d' = x_2' - x_1'$

and in which the question to be answered is whether d is significantly different from d'.

Sometimes variations of the ideal model of the controlled experiment are made necessary by factors over which the experimenter has no control. For example, the research worker may wish to determine whether the mean attitude test score for soldiers stationed overseas differs from the mean score for soldiers in domestic areas. In this case, he may have available only the values for x_2 and x_1'. An attempt is then made to control other factors, such as length of service, branch of service, and so forth, which might influence the comparison by matching the groups on these factors. This and other variations are discussed by Stouffer. Although Stouffer's orientation is toward social-psychological research, the designs he discusses are frequently used in educational and clinical psychology and his discussion applies equally well to these areas.

Ex post facto studies.—Another type of study design used in clinical, educational, and social psychology is the "ex post facto" study described in detail by Chapin (2) in 1947. As an example of this design, consider Christiansen's study (3) in which it was desired to compare the economic adjustment of 1,130 high school graduates (experimental group) and non high school subjects (control group). Since many variables other than high school education might be expected to influence the comparison, members of the experi-

[1] This review covers the period from June, 1949 to approximately May, 1950.

mental group were matched with the control group on as many of these variables as possible. It was found, however, that when the groups were matched on five factors, the experimental group was reduced to 200 subjects. Matching the groups on a sixth factor further reduced the size of the experimental group to 145 subjects. When individuals instead of groups were matched on six factors, only 23 successful matches were made.

The expectation, of course, in the ex post facto design is that factors which are equated for the two groups will not influence the comparison on the variable of investigative interest. That this may not be the case when a large number of subjects are eliminated from the experimental group because successful matches cannot be made, is demonstrated by a study of Freedman & Hawley (4). Freedman (5) reports, for example, that in comparing educational differences between matched groups of migrants and nonmigrants, a significant difference was observed when 58 per cent of the original group of migrants was matched with a control group of nonmigrants, but the difference was not significant when 89 per cent of the original group was matched. In other words, a difference which may be present between subjects easily or readily matched may not be the same as the difference observed between subjects who are more difficult to match.

Experimental designs in studies of repression.—Criticism has also been directed at experimental designs used in laboratory studies of repression. It is Zeller's contention (6) that none of the designs which have been used provides a test of "the hypothesis that repression proper is a process which inhibits recall without, however, destroying the memory for the subject matter, and that the memory can be restored by the elimination of the repression factor." He proposes the following experimental design for testing this hypothesis:

	Control Group			Experimental Group
I.	Step 1.	Learning	=	Learning
	Step 2.	Retention test	=	Retention test
		Time interval		
II.	Step 3.	Retention test	=	Retention test
	Step 4.	Neutral task		Repression
	Step 5.	Retention test	>	Retention test
		Time interval		
III.	Step 6.	Retention test	>	Retention test
	Step 7.	Neutral task		Removal of repression
	Step 8.	Retention test	=	Retention test

If the hypothesis, as stated, is substantiated, then the results indicated above may be expected.

Analysis of Variance

Comparison of individual means.—Webb & Lemmon (7) have pointed out that the usual precautions given by most texts with respect to making sepa-

rate t tests for the differences between pairs of means, when the F test, as ordinarily made in the analysis of variance, indicates that the null hypothesis is tenable, may not necessarily apply in special cases. They suggest that this is true when the experimental conditions are ordered in some fashion and bear some functional relationship to another variable X. Five such functional relationships are cited where the t and F tests lead to conflicting statements concerning significance, but where Webb & Lemmon believe that the t test would be justified, despite the nonsignificance of F.

Fisher (8) and others following Fisher's lead have pointed to the subtle distinction between experiments in which individual comparisons are planned and where they are made after the fact. In the latter case, it still seems sound advice to be wary of individual comparisons. If the experimenter has some advance theory or knowledge concerning the nature of the functional relationships mentioned by Webb & Lemmon, individual comparisons would undoubtedly be planned in advance or, as seems more likely, such experiments would be analyzed more adequately by regression techniques, as Webb & Lemmon note, rather than by the usual simple two-part analysis of variance.

A significant paper has been published by Tukey (9) which also deals with the problem of the comparison of individual means in the analysis of variance. His procedure is proposed for further comparisons to be made after the F test indicates that a set of means vary more than can reasonably be expected in random sampling from a common population. This is a problem which many an experimenter working with the analysis of variance has had to face and one for which no very satisfactory answer was previously available.

Specifically, let us say that we have seven experimental conditions replicated 10 times. We thus have seven means and an unbiased estimate of their variance given by 1/10 the error mean square which is based upon 63 degrees of freedom. The F test indicates that the means are not homogeneous. Must we stop here? Can we make individual comparisons which will permit us to classify the means into subgroups which are alike among themselves but different from other subgroups? Various solutions to this problem have been proposed, but the problem is not a simple one and many of the solutions appear to be based solely upon intuition. The procedure which Tukey describes, on the other hand, is quantitative, direct, reasonably simple, and possesses the advantage of not requiring any additional tables other than those customarily available.

When real differences are present among a set of means, Tukey states that one or all of the following conditions may be observed: (*a*) there is a wide gap between adjacent means when they are arranged in order of magnitude; (*b*) one of the means is a "straggler"; (*c*) the means taken as a group show excessive variability. He proposes that we apply three tests in order to detect these conditions.

The first test is applied by taking the tabled value of t at some defined

level of significance, say $t_{.05}$, for the degrees of freedom available and then solving for

$$\text{Significant gap} = (t_{.05})(\sqrt{2})(s_{\bar{x}}) \qquad \text{I}$$

where $s_{\bar{x}}$ is the standard error of a single mean based upon the common or pooled estimate of error. Any gap between means larger than that obtained by formula I above is taken as a group boundary. If the application of I separates the means into groups such that no group has more than two means the process terminates.

If, on the other hand, groups of three or more means exist after the application of I, then for each subgroup of three or more means, we find the grand mean $\bar{m}$, the most straggling mean m_1, and the difference between these two divided by $s_{\bar{x}}$. These ratios can then be translated into approximate unit normal deviates by finding

$$z = \frac{\left(\frac{m_1 - \bar{m}}{s_{\bar{x}}}\right) - \frac{6}{5}\log_{10} k}{3\left(\frac{1}{4} + \frac{1}{n}\right)} \qquad \text{II}$$

if k (the number of means in the group) is greater than three, or

$$z = \frac{\left(\frac{m_1 - \bar{m}}{s_{\bar{x}}}\right) - \frac{1}{2}}{3\left(\frac{1}{4} + \frac{1}{n}\right)} \qquad \text{III}$$

if there are only three means in the group.

The value of n in II and III is the number of degrees of freedom associated with the estimate of the error mean square, i.e., 63 in the example cited. Since the value of z significant at the 5 per cent level for the unit normal curve is 1.96, any straggling mean in a subgroup which yields a value of z of 1.96 or greater is separated from the subgroup. If any means are separated from a subgroup and the subgroup still contains three or more means, the process is repeated until no additional means are separated. All means separated on the same side are considered as belonging in a new subgroup.

To apply the third criterion, i.e., to determine excessive variability in any remaining subgroup with three or more means, the sum of squares of the deviations of the individual means m from the subgroup mean $\bar{m}$ is found. Dividing this sum of squares by one less than the number of means involved will yield a mean square. Then

$$F = \frac{\frac{\Sigma (m - \bar{m})^2}{k - 1}}{s_{\bar{x}}^2} \qquad \text{IV}$$

where $s_{\bar{x}}^2$ is the square of the standard error in I above. The degrees of freedom available for evaluating F will be $k-1$ for the numerator and n (as defined in II and III above) for the denominator.

Calculation of interactions.—A method for calculating the sums of squares for second- and higher-order interactions in the analysis of variance has been described by Edwards & Horst (10). In a factorial design, for example, with A varied in four ways, B varied in three ways, and C varied in two ways, it is possible to set up subtables for the A and B variables under the separate C conditions, C_1 and C_2. Then the first-order interactions between A and B calculated under the separate C conditions may be designated $C_1(A \times B)$ and $C_2(A \times B)$. The first-order interaction $A \times B$ may be obtained in the usual way from the $A \times B$ table for the combined C conditions. Then the second-order interaction

$$A \times B \times C = \Sigma C(A \times B) - A \times B \qquad \text{V}$$

or, in general

$$\begin{aligned} A \times B \times C &= \Sigma C(A \times B) - A \times B \\ A \times B \times C &= \Sigma B(A \times C) - A \times C \\ A \times B \times C &= \Sigma A(B \times C) - B \times C \end{aligned} \qquad \text{VI}$$

Edwards & Horst also show that a third-order interaction will be given by

$$\begin{aligned} A \times B \times C \times D &= \Sigma A(B \times C \times D) - B \times C \times D \\ A \times B \times C \times D &= \Sigma B(A \times C \times D) - A \times C \times D \\ A \times B \times C \times D &= \Sigma C(A \times B \times D) - A \times B \times D \\ A \times B \times C \times D &= \Sigma D(A \times B \times C) - A \times B \times C \end{aligned} \qquad \text{VII}$$

and that a similar series of equations can be written for any higher-order interaction. They give a numerical example showing the necessary calculations and a general proof for the series of equations.

Nonadditivity and transformations.—The problem of nonadditivity in the analysis of variance has been discussed previously by Eisenhart (11). In an interesting paper, Tukey (12) has proposed a method for determining the presence of nonadditivity in experimental designs in which the data are arranged in the form of a row by column table. In this design, the interaction between the rows and columns is the basis for the measure of error against which the significance of the row and column mean squares are tested.

Tukey demonstrates how it is possible in this design to isolate a sum of squares with a single degree of freedom from the row by column interaction sum of squares. This sum of squares is designated as the sum of squares for nonadditivity. We thus have the row by column interaction sum of squares analyzed into two component parts. One part is that for nonadditivity and is associated with one degree of freedom, and the second is the remainder with $(r-1)(c-1)-1$ degrees of freedom. Each of these sums of squares when divided by its degrees of freedom, yields a mean square.

Tukey points out that the mean square for nonadditivity may be expected to be large, in comparison with that for remainder, under two conditions:

"(*a*) when one or more observations are unusually discrepant;

(*b*) when the analysis has been conducted in terms where the effects of rows and columns are not additive."

Consider the illustrative data in Table I. The usual analysis of variance

TABLE I

ILLUSTRATIVE EXAMPLE OF AN $r \times c$ TABLE WITH NONADDITIVITY*

Cell entries and sums				
14	2	1	2	19
2	0	2	2	6
2	1	5	0	8
18	3	8	4	33

* From Tukey (12).

TABLE II

SUMMARY OF THE ANALYSIS OF VARIANCE OF THE DATA IN TABLE I*

Source of variation	Sum of squares	*df*	Mean square
Rows	24.5	2	12.2
Columns	46.9	3	15.6
Rows × Columns	84.8	6	14.1
Total	156.2	11	

* From Tukey (12).

would be summarized as shown in Table II. Then the sum of squares for nonadditivity may be obtained by calculating

$$\frac{\left[\sum_{1}^{N} (X_{ij})(\overline{X}_{i.} - \overline{X}_{..})(\overline{X}_{.j} - \overline{X}_{..})\right]^2}{\sum_{1}^{r} (\overline{X}_{i.} - \overline{X}_{..})^2 \sum_{1}^{c} (\overline{X}_{.j} - \overline{X}_{..})^2} \qquad \text{VIII}$$

where X_{ij} is the cell entry in the ith row and the jth column, $\overline{X}_{i.}$ is the mean of the ith row, $\overline{X}_{.j}$ is the mean of the jth column, and $\overline{X}_{..}$ is the grand mean. For the data given in Table I, the sum of squares for nonadditivity is equal to

$$(68.8)^2/(6.08)^2(15.44)^2 = 50.9.$$

If the sum of squares for nonadditivity is subtracted from the sum of squares for interaction, the analysis of variance for the illustrative example is as shown in Table III. The mean square for nonadditivity may be tested for significance by taking $F = 50.9/6.8 = 7.5$ with 1 and 5 degrees of freedom —a significant value.

Tukey does not recommend testing the mean squares for rows and columns against the mean square for remainder when the mean square for nonadditivity is significant. Instead, he suggests that the data be examined to

TABLE III

ANALYSIS OF VARIANCE OF THE DATA OF TABLE I WITH ONE DEGREE OF FREEDOM FOR NONADDITIVITY*

Source of variation	Sum of squares	*df*	Mean square
Rows	24.5	2	12.2
Columns	46.9	3	15.6
Nonadditivity	50.9	1	50.9
Remainder	33.9	5	6.8
Total	156.2	11	

* From Tukey (12).

determine whether the nonadditivity is the result of the analysis of the data in the wrong form (scale) or whether it is the result of one or two unusually discrepant observations. In case no unusually discrepant observations are found, consideration may be given to the problem of the kind of transformation required to restore additivity. The data may be examined in the following way: Multiply the entries in a given row by the corresponding deviations of the column means and sum for each row; plot these sums of products against the corresponding row means. Two *s* limits may be established by taking

$$\begin{pmatrix}\text{Average of the}\\ \text{sum of}\\ \text{cross products}\end{pmatrix} \pm 2\sqrt{\begin{pmatrix}\text{Sum of squares of}\\ \text{deviations of}\\ \text{column means}\end{pmatrix}\begin{pmatrix}\text{Mean square}\\ \text{for}\\ \text{remainder}\end{pmatrix}}. \qquad \text{IX}$$

In the illustrative example, IX becomes

$$15.33 \pm 2\sqrt{(15.44)(6.8)}.$$

A single unusually discrepant observation will tend to be reflected by one point high or low and the others distributed around a nearly horizontal regression line. An analysis in the wrong terms will tend to be reflected by a slanting regression line.

In case the plot reveals that an analysis has been made in the wrong terms, the sign of the sum of cross products in the numerator of VIII should be examined. The type of transformation which may then be appropriate for the data may be determined by referring the sign of this sum of products to Table IV.

TABLE IV

SIGN OF FINAL SUM OF PRODUCTS WHEN CERTAIN TRANSFORMATIONS ARE APPROPRIATE*
(Values of x or $x+a$ Nonnegative)

Transformed values which are additive	Conditions needed	Sign when x is analyzed	Important special cases
x^p	$0 \geq p < 1$	+	$\sqrt{x}$, $\sqrt{x+1}$
or	$p=1$	0	(x)
$(x+a)^p$	$1<p$	−	x^2, x^3
$\log (x+a)$	(none)	−	$\log x$, $\log (1+x)$

* From Tukey (12).

That nonadditivity, as described by Tukey, may prevail with respect to certain measures of the galvanic skin response has been shown by Haggard (13). This was found to be true of resistance change and conductance change. The transformation which seemed to be appropriate was the logarithm of the measure. In general, however, the log conductance change scale was found to be a more satisfactory measure than the log resistance change scale in that it best satisfied "the criteria of additivity, normality, homogeneity of variances, independence of means and variances, randomness, and maximal precision."

In a second paper, Haggard (14) showed that if four experimentalists performed the same experiment, obtaining the same raw data, but with each using a different one of the four mentioned measures of the galvanic skin response, the conclusions drawn by the different investigators would not agree if such conclusions were based upon the analysis of variance applied to the data. He again recommends the use of the log conductance change scale as best meeting the requirements of the analysis of variance, and he points out that "those measures which violate the basic assumptions underlying the analysis of variance technique" may be assumed to "yield biased F-values."

Homogeneity of variance in the Latin square design.—Edwards (15) has pointed out the need to consider homogeneity of variance in two particular cases of the Latin square design, described earlier by Grant (16). These cases concern (*a*) the replication of an experiment with a series of independently drawn Latin squares and (*b*) the replication of an experiment by repeating the same Latin square. Each of these designs involves the pooling of sums of squares and degrees of freedom to arrive at a common estimate of the error variance. This pooling is proper if the separate estimates of the error variance which might be obtained from the replications of the experiment are homogeneous. Homogeneity of variance may be tested by means of Bartlett's test (17). If the hypothesis of homogeneity of variance is tenable, the

pooling is proper. If the hypothesis is rejected, an unreliable experimental technique is indicated. Numerical examples of the two analyses are presented in the article.

Regression.—Carter (18) has discussed a method of analysis appropriate for data which are obtained from a series of parallel samples with observations of two or more variables in each sample and in which the corresponding members of the different samples have certain characteristics in common. For example, in psychological research, measurements of two variables X and Y may be made on the same set of individuals at two different times, as indicated below:

Individuals	Test 1		Test 2	
1	X_{11}	Y_{11}	X_{12}	Y_{12}
2	X_{21}	Y_{21}	X_{22}	Y_{22}
3	X_{31}	Y_{31}	X_{32}	Y_{32}
.	.	.	.	.
.	.	.	.	.
.	.	.	.	.
n	X_{n1}	Y_{n1}	X_{n2}	Y_{n2}

Suppose it is desired to test the homogeneity of regression in the two samples and also to determine whether the regression is significant. Since each individual contributes one pair of measurements in each sample (test period), the samples are not independent and the usual tests of homogeneity and significance are not applicable. Carter describes the method of variance analysis and tests of significance appropriate to this design and illustrates the arithmetic involved. The method is general and is applicable to experimental designs involving more than two such samples.

Another use of regression in the analysis of variance has been described by Bruner, Postman & Mosteller (19) in a study of reversible perspectives. These investigators point out that the results of experiments on reversible perspectives have, in the past, frequently contradicted each other. They suggest that the contradictions are due in part to the failure to consider or to isolate three sources of variation: (*a*) set, (*b*) subject, and (*c*) sequence. An experimental design, using the analysis of variance, is then described in which these three factors are varied systematically.

Nineteen subjects were tested for reversals for 10 min. under each of three sets of instructions: (*a*) to reverse the figures as rapidly as possible, (*b*) to keep the figures from reversing, (*c*) to permit the figures to reverse at the subject's own rate. These instructions were balanced with respect to order.

The data which Bruner, Postman & Mosteller obtained approximated a Poisson distribution for which the usual assumptions involved in the analysis of variance would be violated. Bartlett (20) has suggested the transformation $\sqrt{X+.5}$ for this case, and the data which Bruner, Postman & Mosteller obtained were analyzed on this scale. Also present in the data was a time sequence regression effect, with the subjects showing smaller means during the successive minutes of the 10 min. testing period. Regressions were computed

separately for each subject for each set accounting for a total of $3 \times 19 = 57$ degrees of freedom. The analysis of variance which they used then takes the form shown in Table V.

TABLE V*

Source of variation	*df*
Subjects	18
Set	2
Subjects × Set	36
Time-sequence regression	57
Residual	456
Total	569

* From Bruner, Postman & Mosteller (19).

Chi Square Technique

Degrees of freedom.—Guest (21) has raised the problem of the number of degrees of freedom available for evaluating χ^2 in the $r \times c$ contingency table. It is his contention that sometimes in opinion poll work only the total N is fixed and, in other cases, only the row or the column marginals, but not necessarily both. Consequently, he believes that the customary number of degrees of freedom given by $(r-1)(c-1)$ should be increased accordingly. This is reminiscent of the familiar Fisher-Pearson controversy.

With respect to this problem, it is worthwhile quoting Kendall [(22) p. 305] who states:

> In 1922 Yule and Fisher gave respectively experimental and theoretical evidence for what is now accepted as the correct method of determining the number of degrees of freedom in a bivariate table; but Pearson himself never seems to have acknowledged the soundness of this method, and some papers written between 1920 and 1930 on this subject are controversial and therefore not to be accepted uncritically.

Fisher [(23) p. 78], in the 1950 edition of his book, still takes the position that the number of degrees of freedom for χ^2 is determined by the number of ways in which the observed series may differ from the theoretical. Since the $(o-e)$ discrepancies must sum to zero for each row and column of the $r \times c$ table, it is obvious that the usually accepted rule of $(r-1)(c-1)$ will apply.

Partitioning chi square in the $r \times c$ table.—Lancaster (24) has shown that χ^2 for an $r \times c$ table with $(r-1)(c-1)$ degrees of freedom can be partitioned into the χ^2 values for $(r-1)(c-1)$ fourfold tables, each with one degree of freedom. He also shows that the values of χ^2 obtained from each fourfold table will be independent of the values obtained from the other tables. Thus, the sum of these χ^2's will equal χ^2 for the $r \times c$ table with degrees of freedom equal to $(r-1)\ (c-1)$.

Each of the successive fourfold tables can be tested for independence or association by means of the $p_{i.}$ and $p_{.j}$ derived from the local row and column totals instead of the marginals of the complete $r \times c$ table. In the procedure outlined by Lancaster, any comparison involving rows or column totals in the fourfold table is set aside and considered in the adjoining table. Thus, the $r \times c$ table

a_{11}	a_{12}	a_{13}	$a_{1.}$
a_{21}	a_{22}	a_{23}	$a_{2.}$
a_{31}	a_{32}	a_{33}	$a_{3.}$
$a_{.1}$	$a_{.2}$	$a_{.3}$	$a_{..}$

can be divided into the following fourfold tables

a_{11}	a_{12}	R_{12}
a_{21}	a_{22}	R_{22}
C_{21}	C_{22}	T_{22}

R_{12}	a_{13}	$a_{1.}$
R_{22}	a_{23}	$a_{2.}$
T_{22}	C_{23}	T_{23}

C_{21}	C_{22}	T_{22}
a_{31}	a_{32}	R_{32}
$a_{.1}$	$a_{.2}$	T_{32}

T_{22}	C_{23}	T_{23}
R_{32}	a_{33}	$a_{3.}$
T_{32}	$a_{.3}$	$a_{..}$

Similar fourfold tables can be established for any $r \times c$ table, as Lancaster shows. In connection with Lancaster's development, the accompanying note by Irwin (25) should also be consulted.

Misuses of chi square.—A general critique of the use of the χ^2 test in psychological research has been provided by Lewis & Burke (26). It is their contention that while there has been no increased frequency of application of the χ^2 test in psychological research, the number of misapplications has increased. They claim that a survey of papers published in the *Journal of Experimental Psychology* during the years 1944, 1945, and 1946, shows that 14 papers contained one or more applications of the χ^2 test. "The applications in only three of these papers were judged acceptable" by Lewis & Burke. The principal sources of error (or inaccuracy) which they cite are as follows: (*a*) lack of independence among the single events or measures, (*b*) the presence of small theoretical frequencies which invalidate the test, (*c*) failure to take into account the frequencies of nonoccurrence, (*d*) failure to equalize the sum of the observed frequencies and the sum of theoretical frequencies, (*e*) indeterminate theoretical frequencies, (*f*) incorrect or questionable categorizing, (*g*) use of nonfrequency data, (*h*) incorrect determination of the number of degrees of freedom, and (*i*) incorrect computations.

Discriminant Function

Johnson (27) has developed an application of the discriminant function to problems involving a two-way table of qualitative data. In the problem he considered, quantitative values were found for five letter grades which

served to maximize individual differences. Further applications of this procedure might be made in the scoring of interest and personality schedules.

Quenouille (28), using an initial approximation method, has developed a procedure for determining the coefficients of the discriminant function by successive iterations. An earlier method which he has described (29) presented certain difficulties which are avoided in the approximation method.

Psychophysics and Scaling

Johnson (30) has developed a generalized equation for predicting category limens in psychophysical experiments involving a reference scale. In an experimental test of the predictive value of his equation, series of sound stimuli were presented to subjects, each series covering a wide range of frequencies. In some of the series the stimuli were rectangularly distributed, i.e., equally spaced, in others they were skewed. Subjects were instructed to call each sound "high" or "low," setting their own reference standards. The obtained category limens correlated .97 with those predicted by means of Johnson's equation.

The arguments of scale analysts against the use of item analysis techniques are again repeated in an article by Suchman (31). For Suchman and other scale analysts, the problem of scale construction is still primarily one of testing a set of items for reproducibility. The items to be so tested are to be selected intuitively and by agreement, and no help from item analysis techniques is to be permitted.

An interesting application of scaling methods in survey work has been described by Metzner (32). The problem was to select from a set of proposed wordings of a question, the form which would best convey to the respondent the meaning intended by the investigators. Scale values for the alternative wordings were obtained by the method of paired comparisons and the application of Thurstone's Case V (33). By selecting the wording with an intermediate scale value, it was felt that the problem of directional bias was avoided.

Attneave (34) has developed an extension of the method of single stimuli for scaling judgments or ratings which he calls the "method of graded dichotomies." It is commonly found that the distribution of ratings of a stimulus which falls at one or the other extreme of a continuum is skewed because of the limits imposed by the rating scale. Consequently, the obtained mean scale values for such stimuli tend to fall closer to the center of the continuum than would be true of the theoretical values. The method of graded dichotomies "treats the ratings in a manner which rationally avoids the end-distortion" and the assumption of equal scale intervals. Results obtained with the method were found to be comparable with those obtained by the method of paired comparisons.

Psychological Testing

Anastasi (35) has provided a logical analysis of the concept of validity in the interpretation of test scores. She argues that to claim that a test meas-

ures anything more than its criterion is speculation and not amenable to verification. Validity is thus determined by the degree of agreement between performance on a test and some independent criterion of the behavior being studied. It is only when a test is to be used for predictive or diagnostic purposes, however, that the idea of correlation with a criterion is relevant. For example, if the maze learning of rats is under investigation, the maze is not "validated" in terms of success of the rats in avoiding cats outside the laboratory in the everyday world. Instead, the experimenter may argue reasonably "that for the particular principles of behavior which he is investigating, maze-learning is as 'good' a sample of behavior as cat-avoiding, and that he has no more reason for validating the former against the latter than vice versa." In another article concerned with validity of test scores, Cureton (36) has provided a striking demonstration of the fallacy involved in obtaining a validity coefficient from the same data used in making an item analysis.

The thesis that at the present time psychological testing lacks a fundamental psychological rationale with respect to the concepts used in test theory has been developed by Coombs (37). He sketches a theoretical psychological foundation for the derivation of quantitative definitions of various concepts used in testing, including the concepts of homogeneity and reliability. On the basis of his analysis, he contends that the method of single stimuli, ordinarily used in test construction, does not permit the measurement of homogeneity and reliability. Several alternative courses of action are suggested.

Cronbach (38) has published a review of statistical methods applied in studies dealing with Rorschach scores. Some of the common errors which he found had been made in the application of tests of significance to Rorschach data were: (*a*) use of critical ratio and uncorrected χ^2 with small samples, (*b*) use of sample values in the formula for differences between proportions, (*c*) use of formulas for independent samples when matched samples are compared, (*d*) interpretation of P values without regard to the inflation of probabilities involved when hundreds of tests of significance are made or implicitly discarded, and (*e*) acceptance of conclusions when a significant difference is found with an hypothesis based on fluctuations in particular samples.

Cronbach concludes that statistical methods as applied by Rorschach workers have been open to two sources of error: (*a*) the use of erroneous procedures has led to claims of significance and interpretations which are not warranted; (*b*) the failure to apply the most incisive tests of significance has led investigators to reject significant relationships. "So widespread," he adds, "are errors and unhappy choices of statistical procedures that few of the conclusions from statistical studies of the Rorschach test can be trusted."

A method for treating patterns of three independent scores simultaneously has also been described by Cronbach (39). The method permits the comparison of different groups on the pattern of three scores and also a test of significance for differences in the patterns.

Two new textbooks on testing methods have appeared, one by Cronbach (40) and one by Goodenough (41). Both are notable for the degree of emphasis placed upon statistical methods. Goodenough's book, for example, even includes a chapter on the analysis of variance.

Sampling

Stephan (42) has prepared a nontechnical article on sampling problems in research. Olds (43) has shown some of the uses which may be made of city block data for urban areas. Studies relating such data to voting and other behavior are possible. Block data may also be readily used in sampling plans for urban areas.

In some surveys, neither the head of the household nor the household itself may be the sampling unit desired. A random sample of households is obtained, for example, but it is then desired to translate the sample of households into a sample of the adult population. Kish (44) describes a procedure for selecting a single respondent at random from all of the potential respondents in the household.

Lundberg & Larsen (45) have described some of the characteristics of the hard-to-find individuals in sampling surveys. Politz & Simmons (46) have published additional information on their previously published plan (47) for eliminating the need for callbacks. Birnbaum & Sirken (48) have described a technique for the treatment of errors in sampling surveys resulting from the inability to obtain designated respondents who should be in the sample.

Kimball (49) has described an application of sequential sampling methods to the acceptance or rejection of a batch of scored test papers. For example, suppose a large number of papers is scored mechanically. The accuracy of the scoring of the papers is in question and it is not practical to re-score the entire lot. A decision concerning the entire group is to be based upon a detailed examination of a small sample of the papers. A set of papers is selected at random from the entire group and, one at a time, re-scored. Each paper is classified as satisfactory or unsatisfactory in accordance with tolerance limits set by the administrator. After the first paper has been scored a decision is made to (*a*) accept the group, (*b*) reject the group, or (*c*) re-score another paper. If (*c*) is chosen, then another paper is re-scored and the process repeated until decision (*a*) or (*b*) is made. The sample size is not fixed in advance. Tables are included in Kimball's article for applying the sequential sampling method. The method is apt to prove most useful when applied to large groups of 4,000 or more papers.

Miscellaneous Computational Techniques

Using Hotelling's iterative method, (50) Green (51) describes a procedure for finding weights for scores of separate tests such that the composite score will have maximum reliability. Hsü (52) has described a technique for computing the correlation between a quantitative continuous variable and a discrete Poisson series.

Saunders (53) has developed a technique for reducing the number of plots in factor analysis problems when the graphical plane method of rotating axes to simple structure is employed. He first finds the number of test vector termini common to each and every pair of hyperplanes. These figures are then compared with the number to be expected on the basis of chance. Graphs are then plotted of those combinations of factors showing the greatest excess over chance expectation.

Jenkins (54), pointing out that the Thurstone diagrams for determining tetrachoric r are out of print, has devised a short cut method for obtaining the values of tetrachoric r which employs a single chart. The published copy of the chart is too small for actual use, but Jenkins states that copies suitable for use may be obtained upon request.

A nomograph has been published by Reynolds (55) for estimating the correlation coefficient between X and Y when both variables have been corrected for heterogeneity. The method is based upon an approximation formula of Peters & Van Voorhis [(56) pp. 208–10]. Another nomograph has been published by Herfindahl (57) which serves to minimize the calculations involved in finding χ^2 in item analysis procedures.

Cottle (58) has pointed out that the method described by Thurstone [(59) pp. 46–48] for computing the inverse of a matrix lacks clarity at one particular step in the process. For the assistance of others who may wish to use Thurstone's method, Cottle outlines the step which seems to cause difficulty.

Unclassified Reviews and Books

A critical review of the statistics of the Kinsey report on sexual behavior of the male has been published by Wallis (60). He contends that "the basic significance of the findings cannot be securely buttressed by the factual material in the volume," and that "the inadequacies in the statistics are such that it is impossible to say that the book has much value beyond its role in opening a broad and important field."

A review of the differences and similarities in frequency and nonfrequency theories of probability has been presented in an extremely interesting fashion by Kendall (61).

Burt (62) has reviewed the work on the isolation of factors through factor analysis methods up to the present time. His article is not a technical review of methods, but rather it emphasizes the results achieved through factor techniques.

Cronbach (63) has reviewed the evidence concerning the influence of "test-taking habits" on performance. Such habits are designated by Cronbach as "response sets." Evidence is cited to show that response sets are not only operative in psychological testing, but may also be present in experimental and laboratory studies involving psychophysical judgments. He also cites the resemblance between response sets as inferred from statistical data and the "learning sets" found experimentally by Harlow (64).

Statistical techniques and the applications of these techniques to bio-

logical research have been reviewed by Cochran (65). His review covers the design of experiments, sampling problems, and techniques for the analysis of biological data.

Dixon & Massey (66) have published an "experimental" edition of a new text in general statistical methods. The book is designed for a course which students in various fields would take prior to statistical training in specialized areas. Examples and problems have been taken from the fields of psychology, agriculture, biology, education, economics, and industry.

The second edition of Guilford's elementary text (67) is a revised and greatly expanded treatment of the material originally presented in the first edition. More attention has been given to the analysis of variance and to problems of the reliability and validity of test scores. Greater emphasis has also been placed upon small sample theory. A brief introduction to factor analysis has also been included along with a chapter on scaling procedures.

The eleventh edition of Fisher's classical text (23) was published during the past year. It differs, however, in no discernible detail from the tenth edition.

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GERONTOLOGY (LATER MATURITY)[1]

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All living organisms undergo progressive changes in structure and function with the passage of time. These changes, which begin at conception and end only at death, constitute the subject matter of gerontology. Thus, in the broadest sense, problems of growth, development, and maturation are as much a part of gerontology as are those of atrophy, degeneration, and decline. However, the present chapter will be confined to the changes that occur in later maturity and senescence. Since age may be studied as the independent variable in any problem, one might expect to find a sub-heading entitled, "Age changes" in each chapter of this book. In fact, this was the case in certain chapters of Volume 1 of the *Annual Review of Psychology* (1, 2). This scattering of information and observations has made it difficult to obtain an overall evaluation of the present status of our knowledge in the important field of gerontology. The most comprehensive reviews of psychological aspects of aging are those of Kubo (3), Miles (4, 5, 6), Miles & Miles (7), Hofstätter (8), Wechsler (9), Kuhlen (10, 11), Kaplan (12, 13), Lorge (14), Donahue (15), Anderson (16), and Granick (17).

Although the present review is concerned primarily with current research on the psychological aspects of aging, references to earlier studies will be included. For purposes of discussion, the material has been organized under the following major headings: Perception, Motor Responses, Intellectual Functions, and Personality and Social Adjustment.

Perception

Hearing.—Quantitative measurements of auditory acuity have demonstrated that hearing loss with increasing age is greater in males than in females and is more severe for frequencies above 1,024 c.p.s. than for frequencies below this value. Bunch (18) has reported average audiometer curves over the range of 32 c.p.s. to 16,384 c.p.s. by decades from 20 years to 60 years. For tones with frequencies up to and including 512 c.p.s., the total impairment with aging was slight. These findings were confirmed by Ciocco (19) who analyzed his data by tabulating the incidence of types of audiograms instead of by averages for age groups. His contention (20) that the decline in acuity of hearing by bone conduction is greater than by air conduction has not been subjected to further tests. Montgomery (21), as well as Himes & Webster (22), have observed increased incidence of hearing impairment with age among unselected groups attending the World's Fair and

[1] This review covers the period from approximately January, 1947 to June, 1950. However, in order to provide adequate background for certain topics, somewhat more than the customary use of articles prior to this period was made.

the San Diego County Fair. Saltzman & Eversden (23) analyzed the various types of aural impairment in a sample of 1,000 clinic patients of both sexes between the ages of 5 and 71 years. Beasley (24) has shown that the incidence of hearing impairment is greater in the lower socio-economic groups than in the higher. The impairment for the higher tones in people over 50 is of psychological significance, since the perception of the higher frequencies plays an important role in the understanding of conversation when several people are speaking at the same time. Further studies on the effect of age on the perception of speech are needed.

Vision.—The increased presbyopia of older people is well known and has received confirmation in many early studies summarized by Friedenwald (25). Birren & Bick (26) reported visual acuity measurements on 107 subjects between the ages of 40 and 83 years. Visual acuity diminished with age and was correlated with the degree of retinal and macular degeneration observed in ophthalmological examinations. Bernstein (27) found that the change in refraction of the human eye with age follows a logarithmic function. Data were obtained in 344 subjects between the ages of 40 and 65 years, who had been followed by periodic eye examinations (4 to 7 in number for a period of 5 to 14 years). The goodness of fit was such that from the knowledge of four or five prescriptions between the ages of 40 and 50 years a close prediction of refraction at the age of 65 could be made in individual cases.

Age changes in phoria have been reported by Hirsch, Alpern & Schultz (28), who tested 1,192 subjects between the ages of 6 and 50 years. The average phoria at 6 m. became by degrees more esophoric with age, while the average phoria at 40 cm. became gradually more exophoric. Although the age trends, being within $\pm$ two S.D. of the distribution of the total population measurements ($r = .08$ to $.20$), have little clinical significance, they have theoretical value in interpreting the mechanism of accommodation. Scobee & Green (29) reported similar results for males but a reversed effect in females. Mellick (30) has summarized previous observations on normal values of convergence and reported data on 561 subjects. No significant age trends were found in his data when grouped into 10-year categories (last age category, 60 plus).

In a small series of subjects, Mann & Sharpley (31) have observed a reduction in the mean angular radii of the rod field of the dark adapted eye from 60° in the 10 to 30 year-old group to 40° in the 60 to 70 year-old group. The minimum light threshold of the dark adapted eye increases significantly with age (32, 33). Robertson & Yudkin (32) attribute the diminution in sensitivity with age to reduction in pupil size. This explanation cannot be accepted, since Birren, Bick & Fox (33) found this variable had no significant influence on the mean thresholds. In their study of 130 male subjects between the ages of 18 and 83 years, the decline in sensitivity was most marked in subjects beyond the age of 60. In addition to loss of sensitivity, the range of individual differences increased significantly. Comparison between 49 subjects chosen from the Home for the Aged and 43 subjects living in the community and attending a recreation center for the elderly failed to disclose

any significant differences in age effects. Attempts to increase the light sensitivity in aged subject by the administration of vitamin A were unsuccessful. Birren, Bick & Yiengst (34) administered 100,000 units of vitamin A per day to 12 aged subjects over a 38-day period and to four subjects over a 76-day period without producing any significant change in serum vitamin A content or in light threshold. Ophthalmologic examination of these subjects revealed a high prevalence of changes in the retina, macula, and transmission systems of the older eye. For instance, among 39 of the subjects between the ages of 70 and 83 years, none were free of retinal and macular degeneration and lenticular sclerosis.

Changes in the structure of the eye with increasing age have been described by Fischer (35), Lloyd (36), and Mayer (37). Mayer (37) points out that certain anomalies of the eye may be present at birth, but because they do not interfere with this function they may persist and be recognized only in the adult aging eye, as for example, blood vessels in the primary vitreous humor.

The conclusion reached by Tiffin (38) "that after age 45 both sexes show a loss in color vision" is difficult to accept. The change in his studies is not pronounced and is irregular (38, 39). Boice, Tinker & Paterson (40) could find no evidence of deterioration in color discrimination between the ages of 20 and 59 in a group of 236 faculty men tested using a combination of the Ishihara test and the American Optical Company test. An abnormally high incidence of color blindness, namely, 15 to 25 per cent, was found in a small sample of men over the age of 60. Smith (41) also could find no changes in the ability to discriminate colors by matching among subjects between the ages of 30 and 64; but beyond this age, there was a definite drop in ability to discriminate. The author believes that age differences in color-matching ability are more a function of attitudes and learning factors than of changes in receptor physiology in the aged. In view of the lack of agreement between the studies quoted and the apparent discontinuity in age changes in this function, it is apparent that longitudinal studies, particularly between the ages of 55 and 65, are required.

Although there is little doubt that critical flicker fusion frequency (CFF) is significantly lower in 60 to 80 year olds than in 19 to 30 year olds (42, 43, 44), the functional relationship between CFF and age has not been described. The most extensive study reported to date is that of Misiak (44), who compared 50 subjects between the ages of 19 and 30 years (25 males and 25 females) with 50 subjects between the ages of 63 and 87 years (25 males and 25 females). The average values fell from 48.01 and 46.18 for young males and females, respectively, to 37.07 and 39.58 for the old males and females, respectively. Some individuals in the old age group showed performances superior to that of the average of the young group. The investigator noted a marked increase in variability of response with increasing age. At the present time it is impossible to evaluate the significance of these results: partly because we do not know how much of the flicker fusion is peripheral and how much is central, and partly because of technical problems in methodology.

Many variables, such as pupil size, degree of dark adaptation of the eye, intensity and wave length characteristics of light source, as well as the light-dark characteristics of the cycle used as the stimulus, have not been adequately controlled in the studies thus far reported. This field requires further, carefully controlled experiments.

Weston (45) has reported a series of interesting observations showing a decline in the quickness of visual perception over a five-year period in individuals between the ages of 20 and 45. The experiments were carried out by experienced observers who identified the gap in a series of Landolt rings. Correction for the time required in marking the orientation of the gap was made so that the time of performance represented the rate of accomplishing the visual discrimination. Comparisons were made between performances in the same subjects after a five-year interval. In all cases, a significant decrement in performance was observed. The rate of decrement was greater in the older subjects and was more apparent at lower levels of illumination (0.5 lumens per sq. ft.) than at the higher illumination (500 lumens per sq. ft.). These results are in accord with the findings of Bouma (46) that contrast sensitivity as a function of age diminishes from the mid twenties on, and of Mann & Sharpley (31), who have found a tendency of the rod field of the dark adapted eye to contract beyond the age of 30 years. Other complex visual tasks, such as the perception of incomplete pictures, require more time in older subjects (ages 35 to 56) than in young adults, as shown by Verville & Cameron (47).

Other senses.—In recent years, little work has been done on age changes in perception in other sense modalities. Birren (48), in his review of vibratory sensitivity in the aged, points out that although it declines significantly in later life, the interpretation of this impairment is still uncertain. He points out the need for improvements in methods of recording, using increased ranges in frequencies and increased numbers of subjects who are measured under a variety of conditions for adequate evaluation of age changes in this sense modality.

Although it is commonly believed that sensitivity to pain decreases with advancing age, quantitative studies of pain threshold leave much to be desired because of the small number of subjects tested and the possible limitations in the quantitative techniques available. Increased threshold to the pain stimulus using the Hardy-Wolff apparatus has been reported in a small series of untrained old and young subjects by Chapman & Jones (49). However, it is questionable whether the Hardy-Wolff apparatus can yield reliable results using relatively untrained subjects.

Motor Responses

The slowing of motor responses in extreme old age is a common clinical observation. Critchley (50) has described and reviewed the senile disorders of gait and concludes that such changes may be due to many causes at various levels of the nervous system. The speed of a variety of motor responses has also been shown to decline gradually beginning at the age of 40 to 50 years (4, 51, 52). The wide range in individual differences among older subjects is

emphasized by Miles (6, 51), who points out that the performance of the fastest one-third of the older age group was as good as the average performance of the young subjects. The speed of simple and choice reaction time to both sound and light stimuli diminishes significantly with age, as shown by Bellis (53) & Goldfarb (54). The positive correlation between length of reaction time and age was significant even between the ages of 17 and 36 years in the study reported by Forbes (55). Animal studies have shown similar relationships in the rat (56, 57) and the cow (58). The physiological basis for the increased reaction time in the elderly is at present unknown. It is apparently not due to changes in excitability since no significant changes in chronaxie were observed by Bourlière (59) in either rats or humans or by Chieffi & Kirk in old men (60). Preliminary observations by Wagman & Lesse (61) on a small number of subjects past the age of 60 have indicated that there may be a slowing of conduction velocity in the nerve trunk of elderly subjects. Experiments designed to evaluate the synaptic delay in older subjects are needed.

Intellectual Functions

Achievement.—The relationship between age and outstanding achievement has been most extensively considered by Lehman and his co-workers. Using the historical method, the ages at which individuals have made major contributions in art, music, literature, and science have been tabulated (62). The evaluation of literary and artistic excellence is admittedly difficult, nevertheless group estimates have indicated that the quality of literary output tends to fall off somewhat earlier than the quantity as age of the writer increases. Although Lehman & Heidler (63) used the composite judgment of 50 well-known histories of English literature in arriving at this conclusion, there is some doubt as to whether their criterion represents 50 independent judgments, since younger literary critics may well be influenced by the judgments of their seniors. Even though many examples of creative achievement during later maturity and old age can be cited (64), it is apparent that significant contributions in certain fields of science, such as chemistry, physics, biology, and medicine, are less commonly made by individuals over the age of 60 (65, 66). Lehman (67) has also observed that many types of present day leaders—legislative, judicial, diplomatic, military, naval, religious, and educational—are significantly older than were their predecessors who held the same nominal positions. However, this trend may be only the result of increased life expectancy, since many of the positions considered are attained either as a result of age or on the basis of work done at an earlier age.

Visher (68, 69) has analyzed the age distribution of starred scientists listed in *American Men of Science* and reports that most of the 2,607 scientists studied were starred while in their forties, but 67 were not yet 30 whereas 20 were past 70. There was no significant trend of the age at starring between 1903 and 1943.

On the whole, historical evidence tends to support the view of Révész (70) that age in itself is no deterrent to creative achievement.

Intelligence.—There can be no doubt that the average scores attained on intelligence tests diminish with increasing age. The studies on which this conclusion is based have been reviewed in detail elsewhere (14, 17, 71 to 75). This decline in average scores begins in the twenties and continues at an increasing rate up to the age of 60. The extent and the nature of the decline in performance beyond the age of 60 is still uncertain, since adequate samples of adults in this age range have not been systematically tested. It is also clear that the age change in performance varies considerably in different sub-tests. For instance, the decrement is very small on vocabulary, information, and similarity test items. In contrast, there is a significant decline in performance on numerical computation, common sense, opposites, series completion, picture arrangement, digit symbol, and analogies problems (76). Eysenck (73) concludes that the most rapid decline is found in those tests which measure the ability to deduce relations and correlates. Halstead (77) has observed that for older people, the most difficult tests are those wherein the subjects were required to break away from old mental habits and to adapt to unfamiliar situations. Similar conclusions were reached by Raven (78) and Foulds & Raven (79). Since there are a number of factors other than intellectual capacity which may contribute to low performance scores, it is difficult to evaluate the age changes in intelligence. Among these extraneous factors Lorge (80) has emphasized the handicap of aged people in test situations where speed of performance enters into the determination of the score. It is his contention that when allowances are made for perceptual and speed factors, the decrement in performance of older people is very small. Unfamiliarity with test material and the test situation may also play an important part in the lower performance scores of adults. Sensory defects in vision and hearing often preclude the use of currently designed nonverbal test material on older people. Furthermore, many of the testing devices do not contain sufficient material at the lower levels of difficulty (Wechsler-Bellevue) or do not have high enough ceilings to test superior adult ability (81). Tests now available also fail to allow for increased knowledge and experience of elderly people. Thus, before we can make generalizations about the intellectual changes with age, additional test materials and situations which will eliminate some of these difficulties must be devised.

The ability to define words or to recognize synonyms does not seem to change appreciably, at least up to the age of 60 years, in the general population (77, 82, 83, 84). Belief in the stability of vocabulary ability with age has led to wide use of vocabulary test scores as an indication of the intellectual level of elderly persons at earlier ages (85, 86). Recent studies throw some doubt on the validity of this assumption, since Foulds (87) has shown a decrement in vocabulary ability beyond the age of 30 years in the lower 25th percentile of a large population, whereas in the 95th percentile of the same group there was a slight but measurable increment with age up to the fifty-fifth year. This is in accordance with earlier studies reported by Christian & Paterson (88), who found a gradual increase in vocabulary scores up to the age of 60 to 69 in the parents and relatives of University of Minnesota

students. Sorenson (89) and Vernon (90) have also reported slight increases in vocabulary with age. The importance of education in determining vocabulary size in later maturity was shown by Fox & Birren (91), who found no significant correlation in vocabulary size and age or length of institutionalization in a group of 260 institutionalized subjects between the ages of 60 and 89 years, although a correlation of 0.38 was obtained between vocabulary scores and number of years of schooling. Lewinski (92) has also shown a significant correlation between vocabulary scores and educational attainment.

Most of the studies on vocabulary ability have concerned themselves with the number of words recognized or defined by the subject and have not considered the quality of the definitions offered. In a preliminary report, Brown (93) has attempted to evaluate the quality of the vocabulary as well as the quantity and has concluded that quality decreases with age. While there are potential pitfalls in such techniques, it seems that this might offer a fruitful area for future research on aging.

One of the major difficulties in research on intellectual changes with age is that the same test instrument may not be sampling the same mental ability at different ages. Balinsky (94) carried out a factor analysis of test scores from the Wechsler-Bellevue Scale on subjects from six separate age groups bebetween 9 and 59 years. The factors isolated from the intercorrelation of scores on subtests for each of these groups, using the Thurstone centroid method with axis rotation, gave different factor groupings for the six different age categories. In the 9 and 12-year-old groups, a general factor concerned with verbality and a factor concerned with ability to recognize relationships were fairly clear. However, with increasing age (25 to 29 years and 35 to 44 years) the factors became more difficult to identify. In the 50 to 59-year group, the average intercorrelations between subtests increased and there was some evidence that the factors isolated were more homogenous than in the middle-age group. In other words, with increasing age there was apparently an increase in specificity of performance. Other studies utilizing nonverbal tests and extending the observations to ages above 60 should reveal important information with respect to mental organization in old age.

Since the so-called "higher mental functions," such as reasoning, critical thinking, judgment, etc., do not lend themselves to quantitative measurement, most of the beliefs about the effect of age on these functions are based on unquantitated impressions and hunches. Although some standardized tests have been devised for these functions, they have not been applied to adequate samples of subjects above the age of 65. Burton & Joel (95) compared the scores of adults below the ages of 36.9 years with those above that age (there were only four subjects between the ages of 57 and 72 years) and found that higher average scores were made by the younger group than by the older group on the Watson-Glaser Tests of Critical Thinking (96). The subjects tested were applicants for clerical Civil Service positions. It is probable that the older group included a disproportionate number of subjects of lower ability, since successful people over the age of 45 are less likely to be included in this group than persons under 45.

Another promising approach to age changes is the technique utilized by Berg (97) to measure flexibility of thinking. This test, based on the ability to detect changes in the basis for sorting cards according to different criteria, indicated less flexibility among older subjects (age 66) than among young. The entire question of perseveration and suggestibility in older people could well be investigated by the same techniques used in child development studies.

The preoccupation with the average curves showing a decline in many mental performances with age has resulted in overlooking two important findings in this field. The first is the wide individual differences in the decrement of performance with age. The second is the relationship between performance level and age decrement. Several studies have indicated that the age decrement is greatest in the subjects showing lower performance ability, but that in the upper five per cent of the population the decrement is minimal or absent. It is possible that the lack of an age decrement in specially competent individuals is due to the inadequacies of our testing devices, that is, the ceiling of all tests is too low. Although it is true that the average performance of young university professors was on the average better than that of old, Sward (98) was forced to conclude that the bright and successful senescent subjects showed few unambigious signs of psychological decline. The question of whether a significant decline in performance occurs with age in superior adults is of considerable social importance and should be intensively investigated. As pointed out by Eysenck (73), one of the serious limitations in our knowledge of intellectual changes with age is the fact that, with the exception of the studies by Miles (99) and Heim & Wallace (100), none of our observations are based on repeated serial tests of the same individual.

Memory and learning.—Although there is, on the average, some deterioration of memory and learning ability with age, it is important to note that the magnitude of the defect is a function of the material to be learned. Thus, Gilbert (101) compared the scores in 11 different types of memory tests in 174 subjects between the ages of 60 and 69 years with 174 subjects between the ages of 20 and 29 years. The two groups of subjects were individually matched on the basis of vocabulary test scores. The smallest difference between the two groups was in visual memory for digits (8.25 per cent), whereas the greatest loss (60.4 per cent) was reported for the retention of Turkish-English vocabulary items. It is of considerable interest to note that a loss of only 32 per cent was observed in the brightest five per cent of the group as compared with 60 per cent loss in the total group in the latter test. Ruch (102) has reported findings of a similar nature in a group of subjects between the ages of 12 and 82 years chosen from the upper socio-economic groups. In addition, tests of motor learning were used, such as direct and mirror vision pursuit-meter tests. It is concluded that a differential defect in learning ability, both motor and intellectual, is a function of the amount of reorganization of pre-existing habit patterns that are required. This is an attractive hypothesis, but one which needs verification by setting up experiments to test the effect of varying degrees of interference. Cameron (103)

reported experiments of this kind on 12 senile patients with retention defect. Interpolated activities interfered more with retention in the 12 aged subjects than in the control group (N not stated). Attempts to improve retention in the older subjects by oxygen inhalation and the administration of vitamins proved unsuccessful. However, this method of approach is one which should certainly be further explored. In view of the social and economic importance of retraining programs for middle-aged and elderly people for their utilization in employment, it is surprising that there has been so little experimental work on defining the optimal conditions for learning in middle-aged and older people. With the current interest in learning theory, it would seem that age changes should be included as the independent variable in many of these studies.

Personality and Social Adjustment

The identification, description, and measurement of changes in personal characteristics with age represent problems of considerable social importance which the psychologist is called upon to solve. The identification of age changes in this field adds difficulties to the already complicated task of measuring personal qualities. For instance, even if present techniques for the measurement of personality traits are accepted, there is no way of determining what the old man of 60 was like at the age of 40. Thus, most of the studies reported deal with age differences rather than age changes. Since it is highly probable that those who make themselves available for study at different ages are selected on a different basis, it is obvious that any fundamental contribution to our knowledge about changes in personality with age must await the accumulation of longitudinal data. Although there are many difficulties in longitudinal research, such as expense, selection and maintenance of samples of subjects, stability of the research staff, and possible effects of cultural changes over a long-time period (104), none are unsurmountable. Earlier reviews of age differences in personality traits of adults are those of Miles (6), Kuhlen (10) and Granick (17).

The close interrelations between interests, activities, motives, attitudes, aspirations, and beliefs, and the diverse methods used in their study makes difficult a logical organization of research material in the field of personality and social adjustment. Due to limitations of space, no systematic presentation of the extensive data on behavior aberrations and mental disease in the aged will be attempted. This material has been reviewed by Kaplan (12).

Attitudes and interests.—Experimental results from a number of studies have been used as evidence for the common belief that older people are more conservative in their attitudes, opinions, and beliefs than are younger adults. Thus, Lorge (105) compared scores obtained two weeks apart on a number of Thurstone's Attitude Scales and found higher correlations between the two sets of scores in a group of 20 to 25 year-old subjects than in subjects over 40 years of age. However, no attempt was made in this study to alter the attitudes of the two groups experimentally. Marple (106) attempted to alter expressed opinions of three different age groups by informing the sub-

jects of majority opinion and also of expert opinion on the same topics prior to a second testing. Although there was a greater shift in expressed opinion among the high school pupils than among college students and adults, one cannot assert that the adults were more conservative in their opinions, since younger subjects still in school are perhaps more inclined to accept statements of authority uncritically.

Fisher (107) compared the attitudes and values of two generations (college students and parents) in the same family. Using data from standard questionnaires, he concluded that in the parents conservative and orthodox viewpoints were stronger than the more radical and critical ones. Ruch (108) accepts the conservatism of older people as a fact which he attempts to explain on the basis of the differential decline in learning ability he observed. Gundlach (109) could demonstrate no relationship between age and liberality of political opinion. Other negative results included those of Young (110), Harper (111), and Pace (112). Pollak (113) analyzed data from questions asked in market research programs in population samples varying in size from 200 to 4,600 subjects. The questions asked dealt with the acceptance or rejection of new commercial products, innovations in packaging, and changes in economic, social, or political organization as well as government policies. This material failed to show any conflict between a conservatism of the old and a progressivism of the young. Where there was a conservative majority among the old there was a conservative majority among the young. Although this division between young and old was set at 40 to 45 years, there is obviously need to examine quantitatively the question of conservatism in the aged. Of even greater social importance is the need to examine the effects of various techniques on the alteration of attitudes in both young and old.

Although a number of descriptions of attitudes toward the aged in primitive (114, 115), ancient (116), and modern society have appeared, the application of quantitative techniques to the problem of attitude measurement has not flourished. Two general types of scales might well be developed; the first is concerned with assessing the attitudes of young, middle-aged, and elderly people toward aging as a process, and the second, the measurement of attitudes toward old people.

Personal and social adjustment.—The concept of "adjustment" has played an important role in moulding research in the field of personality. A decade ago there was considerable emphasis on the development of methods for scaling and extracting quantitative scores from questionnaire items. During this period a number of personality schedules found favor among investigators attempting to isolate degrees of "neuroticism" in individuals of different ages. For example, Willoughby (117), using the Thurstone Personality Schedule, found no significant relationship between age and neurotic scores. Similar negative results were reported by Simpson (118), using the same test, and by Gundlach (109) using his own questionnaire. Other studies by Phillips & Greene (119) and Boynton (120) on teachers ranging in age from 16 to 50 years offered no evidence of increasing neurotic tendencies or shifts

toward poorer personality adjustment with increasing age. Gray (121) reported his clinical impressions from answers to a questionnaire given by 2,000 office patients from 10 to 90 years of age. He, also, was unable to obtain evidence of significant shifts in emotionality or instability in the older subjects. Contrary results are reported in later studies by Willoughby (122, 123), who concluded that neuroticism increased significantly between the sixth and eighth decades of life.

On the basis of these studies, it is scarcely possible to conclude that there is an increase in neuroticism or poor personal adjustment with increasing age, at least up to the age of 50 or 60. Whether this is also true at higher ages cannot be stated, since too few subjects have been studied.

In recent years the questionnaire and interview methods have found greater favor than the scaling techniques in the assessment of attitudes and activities of elderly people. The result of a study of 499 men and 759 women by using the Adult Activity Schedule has been reported by Cavan, Burgess, Havighurst & Goldhamer (124). This Inventory, "Your Attitudes and Activities," consists of a series of statements some of which are checked, others answered by "Yes" or "No," while others have a four or five point intensity distribution. Categories covered in the Inventory include health, family, friends, leisure and recreation, clubs and organizations, employment history, financial security, early life, and attitudes. Arbitrary scores were assigned on the basis of comparisons between other groups of older individuals judged by interviewers to be well adjusted or poorly adjusted. The Inventory was sent by mail to a total of 8,441 individuals geographically distributed over the entire United States. The sample was a fairly close representation of the population of the United States 60 years of age and over with the exception that a much smaller representation of rural farm subjects was obtained. Sub-scores for the following categories were calculated: leisure activities (including organizations), religious activities, intimate contacts (friends and family), health, and security. The authors devoted considerable attention to the reliability and validity of the Inventory and conclude that although improvements could be made, both reliability and validity are high enough to warrant use of the scale. For instance, the correlation between ratings of adjustment made by 2 to 5 judges and scores on the schedule range from .53 to .78. In the sample studied, increasing age was associated with a decrease in the amount of close companionship, in participation in many activities as shown by attendance at meetings, in number of hobbies, and in plans for the future. Also there was an increase in physical handicaps, illness and nervousness and a decrease in feeling of satisfaction with health status. Religious activities and dependence upon religion increased with age, whereas feelings of happiness, usefulness, zest, and interest in life decreased. In general, the older subjects responded with lower median attitude scores indicating poorer adjustment. An increased feeling of economic security, despite a lower amount of income, was reported by many of the respondents. Sex differences in adjustment were also apparent; for instance, women reported

more physical handicaps, illness, nervous and neurotic symptoms, and accidents than men. Women have more religious activity and more favorable attitudes toward religion than do men, but were less happy than men.

A number of other studies have compared adjustment scores on the above questionnaire between specified groups of older people. Thus for instance, Havighurst & Shanas (125) obtained data on 28 members of the Fossils Club, a retired men's club in Washington, D. C., composed of former professional or administrative people. Responses were obtained from 30 per cent of the total membership of the club with a mean age of 73 years. This group of subjects showed much better adjustment to old age than that observed in the general population. The important factors seem to be economic security, relatively late retirement, and congenial housing and living arrangements. Studies on retired Y.M.C.A. secretaries and public school teachers have shown a wide range in adjustment patterns (126). Older people living in institutions have also shown lower adjustment scores on this inventory than those living in the community (127, 128). All of these studies suffer from many uncontrolled variables, such as educational background, socio-economic status, cultural pattern of the community, and home influences. They have, however, served to focus attention on the many variables involved in the adequate social adjustment of older people.

The activities of elderly adults are primarily sedentary, such as visiting, reading, listening to the radio, etc., as indicated from interviews and questionnaires (129, 130). Schramm & White (131), who analyzed newspaper reading patterns, have shown a marked decrement in the reading of comics, cartoons, and news pictures with advancing age, whereas the reading of letters to the editor, editorials, economic views increased. The study was based on a sample of 746 interviews of individuals between the ages of 10 and 60 plus years.

Fried (130) found that inactivity in elderly people was six times greater among the lower economic groups than in the higher. The elderly people interviewed complained of inactivity and indicated preference for doing something. A desire for continued activity has important social implications and indicates the need for reeducation both in individuals and in the community with respect to attitudes toward older people. It also seems evident that our educational system has not succeeded in giving individuals inner resources for continued expansion of interests and activities throughout life.

Reports of happiness in elderly people offer conflicting evidence. Early studies by Morgan (132) and Landis (133), based on interview material, led to the conclusion that most people of advanced age considered the happiest period of their lives to have been that between 25 and 45 years. Kuhlen (134), on the other hand, in a study of 300 adults between the ages of 20 and 80, reported that happiness ratings tended to increase up to the 20's and 30's and to decrease thereafter. The primary sources of unhappiness were bereavement and poor health. Though most of the foregoing subjects reported higher degrees of happiness at early ages, it is important to note that only 3 per cent of the aged people interviewed by Gardner (129) (two-thirds being 70

or over) reported that they were unhappy. Furthermore, wide individual differences in happiness ratings were found by Lawton (135). Although happiness is undoubtedly a characteristic of psychological importance, it is a multi-dimensional quality that requires a more precise definition than can be obtained in the gross overall rating approach. Dependence upon retrospective information about happiness in previous decades of life is also hazardous.

Age changes in the source of anxiety and worry have been reported by Kerr, Newman & Sadewic (136). A questionnaire listing 12 potential fields of worry was mailed to 250 successful American psychologists born prior to 1903. Responses were received from 103 between the ages of 46 and 81 years. Over half of the respondents reported appearance and sexual morality as sources of worry when they were in their twenties. Ninety-one per cent of the group reported economic and financial worry most prevalent in their early thirties. Worries most prevalent in the forties were giving up important hopes or ambitions and marital difficulties. Thus, there is some evidence that the pattern of worry or anxiety shifts with age. However, the group chosen for study may not be typical of the population and the worry patterns reported may not be typical of this group, since it is highly probable that persons feeling minimal anxiety failed to return the questionnaire.

Counseling.—The increase in problems of adjustment facing older people discussed in detail by Havighurst (137) serves to emphasize the need for developing adequate counseling services for older people. Vocational counseling is necessary for the readjustment of middle-aged and older workers (138, 139) as well as young and can yield good results (140). However, relatively little work has been done to date in this important area. General counseling services for older people are extremely scarce, although the need is great. Stern (141, 142) has described the kinds of problems encountered in one such counseling center and has indicated some of the special methods that can be used successfully in dealing with older clients.

Fried (130) points out the inability of elderly people to make adjustments in their activity without help, thus emphasizing the need to develop counseling programs for them. The need for psychological preparation for retirement is pointed out by Chandler (143) who sent a questionnaire to 200 persons listed in *Who's Who in America 1946–1947* (one-half of whom were 75 or over and the other half in their forties); 200 alumni of Vassar College in the classes of 1897 and 1922; 100 members of two professional men's clubs of Columbus, Ohio; and 20 emeritus professors of Ohio State University. Replies were received from 120 individuals. Analysis of the replies indicated that few of the middle aged respondents showed much imagination regarding their later years. The activities anticipated included gardening, golf, reading, and travel. Moore (144) has also emphasized the need for aid in preparing for retirement. It is apparent that even in the higher socio-economic levels there is grave need for counseling programs.

Pressey & Simcoe (145) have attempted to describe the characteristics of successful as contrasted with "problem" old people. The material used consisted of case studies prepared by students in psychology classes. Each

student wrote a case study about one old person of his acquaintance who showed satisfactory adjustment and one individual who was a personality problem. A total of 319 successful and 204 "problem" cases were thus obtained for analysis. The differential of the successful people were: continuing usefulness, many social relationships, and maintenance of various activities and interests. It is impossible to draw general conclusions from material of this kind since one cannot evaluate the sampling bias of cases reported or of the material included by the respondent. It is also apparent that the reported characteristics of the successful group were precisely those which would lead to their selection by the student reporting. While studies of this kind may be useful in identifying activities of interest to older people that could be offered through counseling services, it is of limited value in interpreting age changes.

While questionnaire and interview methods undoubtedly furnish information of value, the quantitative methods for evaluating attitudes should not be abandoned. In view of the selective character of reminiscence, it is doubtful whether too much confidence can be placed upon reports of activity, feelings, and attitudes made by older people. It would seem that researchers in this field would do well to follow the lead of Miles and his group in carrying out experimental studies on older people.

Summary

Significant progress has been made in our knowledge of the psychological aspects of aging, particularly with respect to perception, speed of reaction, and mental test performance. A large part of the information available has been derived as a by-product of other studies and the number of systematic studies of aging or aged subjects has been all too few.

Most of the current research on problems of aging is in the area of mental tests and social and personal adjustment. It is apparent that many unsolved problems remain and that there are certain difficulties peculiar to gerontological research. One of these, the problem of sampling, has been discussed by Havighurst (146).

Far too much of our knowledge of elderly people is based on residents of institutions. While these groups may be of invaluable service in the development of techniques or methods, we must ultimately obtain information on the performance abilities of the older individuals who are still living in the community. Since many investigators may be unable to provide random samples of adults for study, it is of considerable importance that the characteristics of the population used be described in such terms that it can be precisely identified. It is also clear that a great deal of work needs to be done in the field of methodology. For instance, many of the important aspects of adult life, such as reasoning, critical thinking, emotional responsiveness, etc., cannot be quantitatively estimated with any degree of precision by the methods now avilable.

New methods for analyzing age trends in individual as well as group data also need to be worked out.

Planned programs for longitudinal studies of middle aged adults from all intellectual, socio-economic, and educational levels are badly needed.

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